Volume XVI. *April, 1903.* *Number 4.*

THE

PHYSICAL REVIEW.

THE NUCLEATION DURING COLD WEATHER.

By C. Barus.

1. The results shown in the accompanying figures were obtained with the same apparatus and by the same method described in *Science* in December, 1902, p. 948. No attempt has been made to reduce the nucleation rigorously to absolute numbers, as it seems ill advised to carry this reduction out at present. Further study of the apertures of coronas will first have to be made, and the apparatus thereafter changed to appropriate plate-glass troughs. Each cylindrical vessel in which coronas are produced is apt to have a coefficient of its own, and the measurement of aperture alone does not at once indicate the nucleation.

The numbers are, however, roughly absolute and reasonably satisfactory as relations; they, therefore, meet every demand which can be made of them at the present preliminary stage of progress.

2. It will be noticed that during the winter months and particularly during excessive cold, the air is very rich in nuclei. One naturally infers that this is to some degree due to the amount of coal burnt at the time. One must be careful not to overestimate the local effect. There are more nuclei per c.c. than can be produced by twenty needle jets playing at high pressure into a ten liter jar. The nucleation is variable with atmospheric conditions under like local conditions as to wind, etc. Moreover, the winds which bring the nuclei come from the highlands, and not from Pawtucket or the thickly inhabited parts of Providence. Indirectly, however, the local effect must be considered (see below § 12).

The present experiments furthermore show that ordinary terrestrial dust properly so-called, has no bearing on the nucleation; for this is so frequently a maximum immediately after a rain when the earth is blanketed with water, or after the earth has been covered with snow or with sleet when there can be no dissemination of dust as such.

3. I do not attempt in this paper to give a detailed discussion of the correlative atmospheric conditions, as this will have to be done with more data in hand. The temperatures and winds (for 8 A. M.) taken from the weather maps for Block Island or Nantucket will often be entered.[1] In some of the figures the changes of temperature δt and barometric pressure δp are also given, with the occurrence of clear weather ⊙, cloudy ● and partly cloudy weather ◑, sunshine S, rain R, snow Sn, haze h, falling temperature $-\vartheta$, c. w. cold wave, etc. References in Fahrenheit or centrigrade are indicated.

In the paper in *Science*, the results were shown as far as November 3 (Fig. 1), and they are reproduced here on the first figure chiefly for comparison with those following. The scale is the same throughout.

4. The feature of the data from November 1–10 (Fig. 2), is the tendency to notched minima at mid-day or in the early afternoon. The maximum on the 11th coincides with northerly winds and a slight drop in temperature, but beyond this the temperature is so nearly constant until the 24th, that reasons for the variations of the figure are not forthcoming. The remarkable rise on the 21st may be noticed. On the 14th and 23d there are mid-day minima, but others are not apparent.

5. During the remainder of November (Fig. 3), rain minima occur on the 25th, 26th and on the end of the 29th. The maximum on the 29th is clearly associated with the drop in temperature accompanying northwesterly winds. There is here a mid-day minimum, but no others.

Throughout December, cold waves associated with northwesterly winds are productive of maxima. After the rain on December 1, the nucleation rapidly rises, accompanying a second drop in tem-

[1] As the work progressed, superfluous data were gradually dropped.

(To face p. 194.)

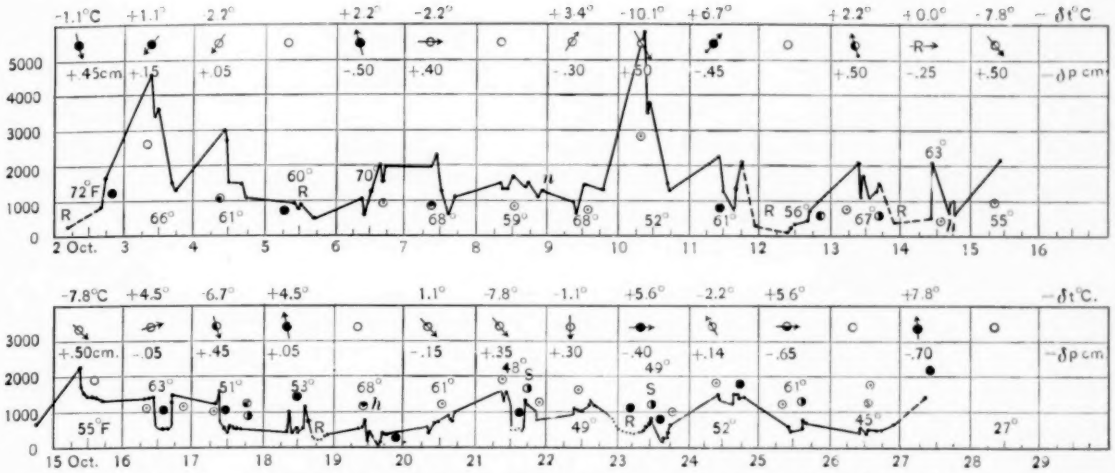


Fig. 1. Number of nuclei per cubic centimeter of atmospheric air or *nucleation* (ordinates) from October 2–28. Wide receiver (diameter 22 cm). Increments of pressure (cms. of mercury), temperature (degrees Centigrade) and winds given above the curves from U. S. W. B. (Block Is.) observations. Local weather data (temperatures in degrees F., at about noon) nearer the curve. Observations from October 2–15 without water-bath. All data inserted in figure at about the time of observation.

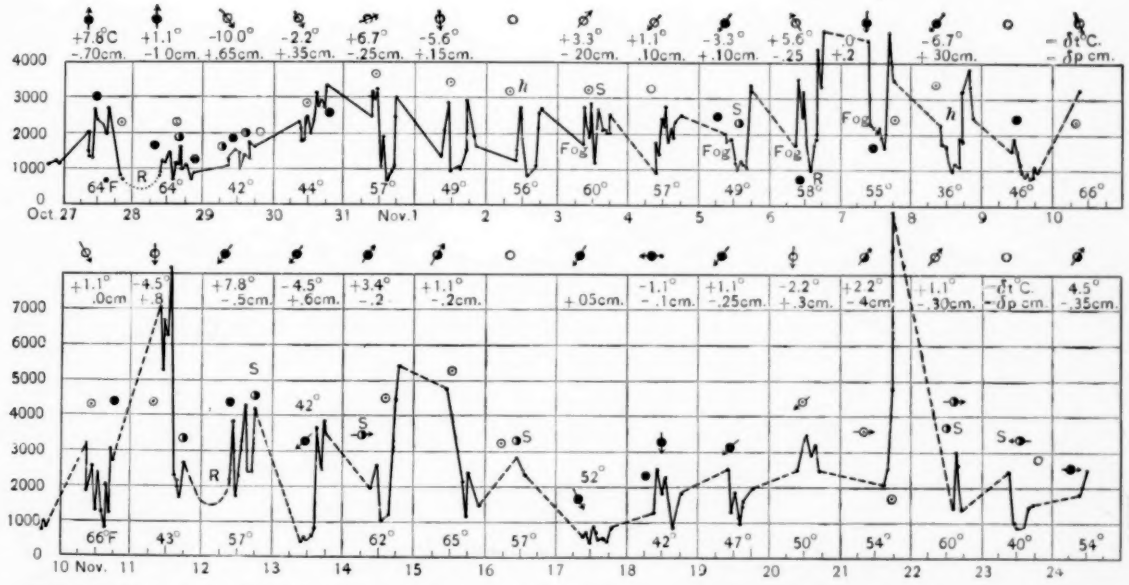


Fig. 2. Nucleation, October 27 to November 24. Tall cylindrical receiver (50 cm. long, 13 cm. diameter). Absolute values too large. Winds, temperature and pressure increments above curves, from U. S. W. B. Local data nearer the curve. Places of data here and elsewhere correspond to the time of observation.

perature and northerly winds. The maximum is moderately sustained until the 3d, where there is a fall into the rain minimum, partial recuperation thereafter and then another drop into the rain on the 5th.

6. This rain soon changes into snow, and from here the nucleation (Fig. 4) of the cold wave begins, lasting from the 6th to the thaw on the 11th. One may note the reduction due to very cold snow on the 7th, and the curious minimum accompanying the gale on the evening of the 8th. The maxima on the 9th and 10th accompany northerly winds with a drop in temperature.

The very remarkable maximum from the 13th to 16th, follows the fall of temperature after the thaw on the 15th, introducing a rain minimum. This interesting region is no doubt referable to the drifting snow storm on the evening of the 13th, the winds remaining northerly. The ground was frozen on the 13th and 14th. It is not due to temperature.

The maxima on the 17th and 20th are less sustained, but they come with temperatures much above freezing and westerly winds. The absence of night observations is a dilemma in these cases. The low nucleation indicated on the 20th continued for several days (20th–23d) beyond the limits of Fig. 4, but is shown on Fig. 5.

6. One may regard it as a probable result of the first four figures that the nucleation in general increases when temperature suddenly decreases, but that temperature is *not* the sole factor involved. To see whether this view would work out in detail, I compared the observatory thermograph data for Providence during the three months, kindly placed at my disposal by my colleague, Professor Upton, with the data for nucleation given in the above figures. A limited degree of similarity was in this way made out, both in the general march of temperature throughout longer intervals and in its detailed variations. Thus the mid-day minima were obviously the counterparts of corresponding temperature maxima. There were, however, many outstanding discrepancies and it seemed probable that much of this would be referable to local differences as the observatory is differently situated and at a considerable distance from the laboratory. Beginning with December 28, therefore, I have been making

daily temperature observations simultaneously with the coronal observations, using a long mercury thermometer placed besides the influx pipe. These temperatures are given at the tops of Figs. 5 and 6, and *positive temperatures* in degrees F., are laid off *downwards* to insure an easier recognition of coincidences, remembering that nucleation maxima and temperature minima correspond.

7. In the fragmentary temperature observations before December 28, Fig. 5, some relations are seen; thus the maximum between December 23 and 25 has a temperature equivalent; the broad minimum on the 22d and mid-day minimum on the 24th correspond, etc.

Turning to the cases under detailed observation, the mid-day minimum on December 28, the fall on December 29, and to a smaller extent the data on December 30 and 31 (in the latter case other factors are also active), agree. The marked minimum on both cases on January 1 and to some extent that of January 2 are especially striking. The rain minimum on January 3 as also the low nucleation on January 4 correspond to slightly variable temperature, etc.

8. In Fig. 6 the general maxima marked c. w. (cold wave) may be noted. The details are similar; one may point out the marked fall on January 11 and the day minimum on January 14. On this figure there is however one good example of opposition; for on January 13 a pronounced minimum of temperature occurs simultaneously with maximum nucleation. Effects such as those on January 13 are not infrequent at certain times and one must guard against overestimating the temperature effect.

SUMMARY AND INFERENCES.

9. The experiments, as a whole, are in the first place to be looked at as a severe test amounting almost to a strain of the method employed. Since both maxima and minima are registered with equal facility at exceedingly low as well as at high atmospheric temperatures, the temperature error of the method itself is not menacing. Rain minima, snow minima and even gale minima have all been recognized. The extreme maxima will require some enlargement of the apparatus since the diameters of the coronas occupy nearly

(To face p. 196.)

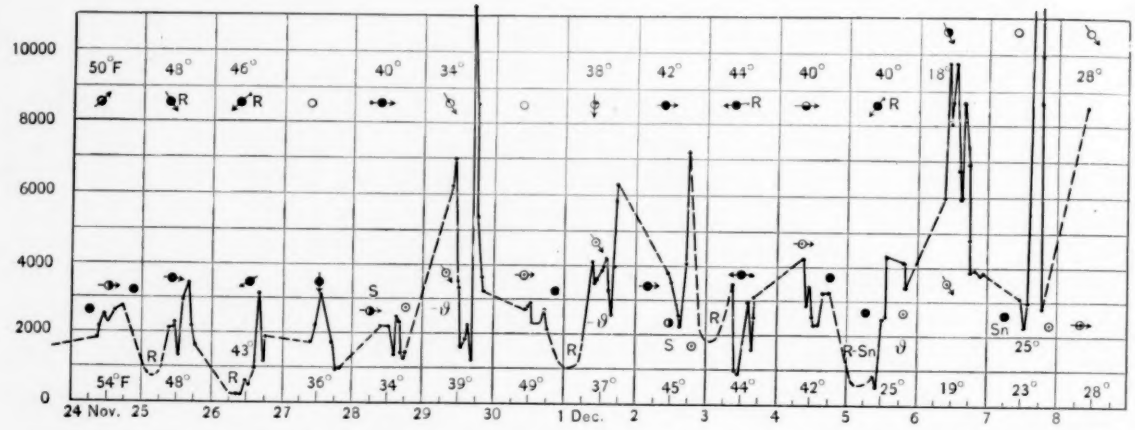


Fig. 3. Nucleation, November 24 to December 8. Temperature (degrees F.) and winds from U. S. W. B., above curve. Local data nearer the curve.

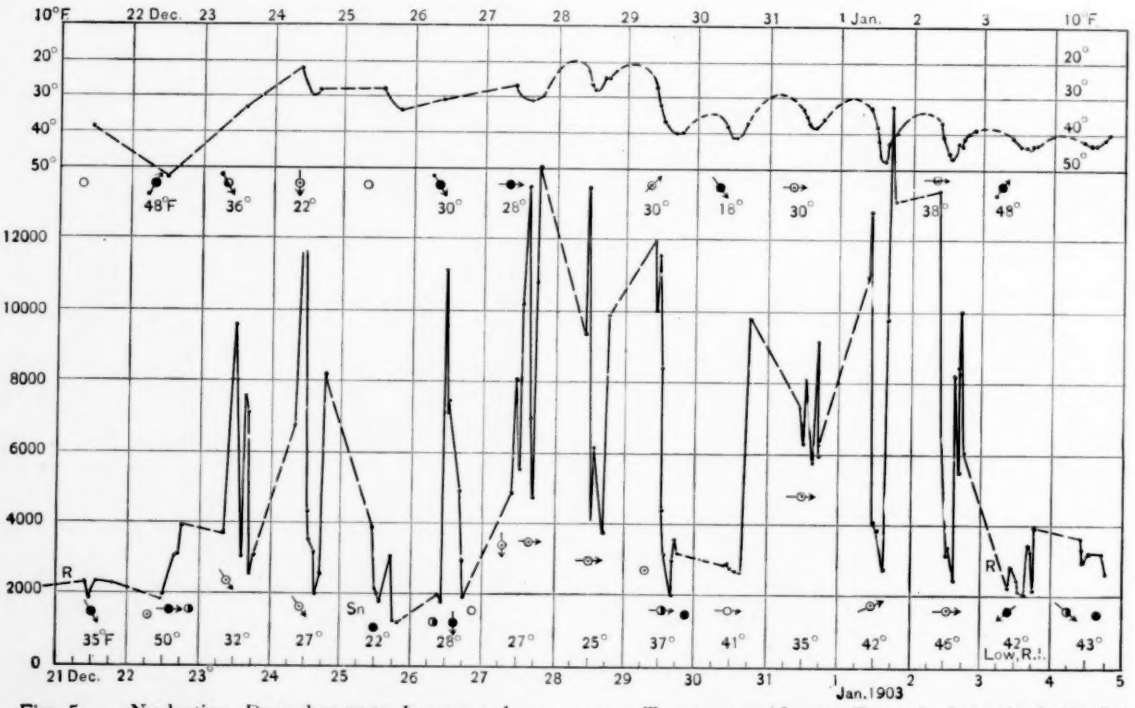


Fig. 5. Nucleation, December 21 to January 4, lower curve. Temperature (degrees F.) and winds (U. S. W. B.) above curve. Local data nearer curve.

Upper curve shows corresponding observed temperatures in degrees F., laid off *positively downward*.

BARUS.

(To face p. 196.)

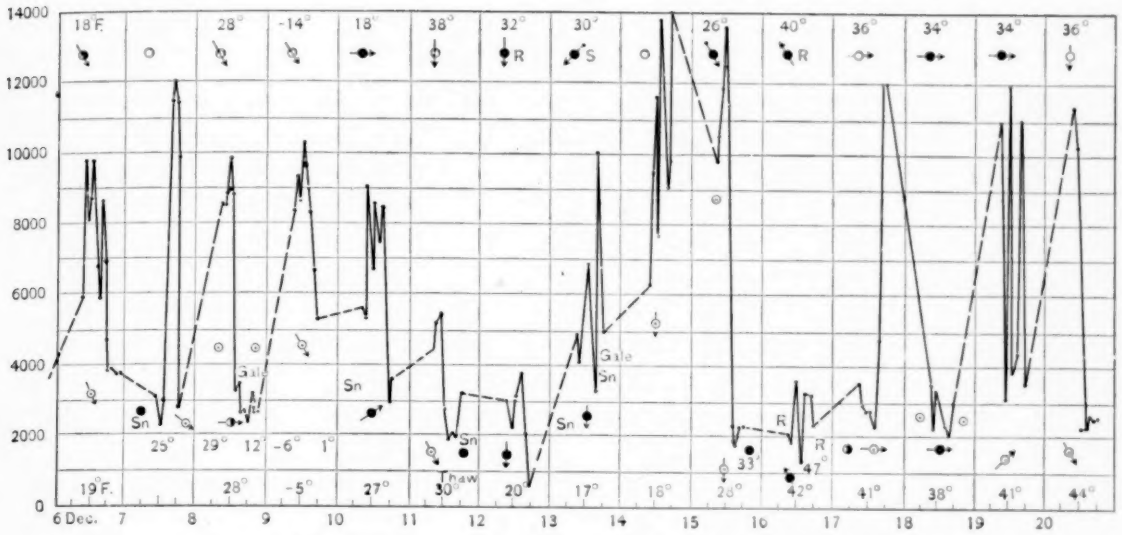


Fig. 4. Nucleation, December 6–20. Data inserted as in the former case.

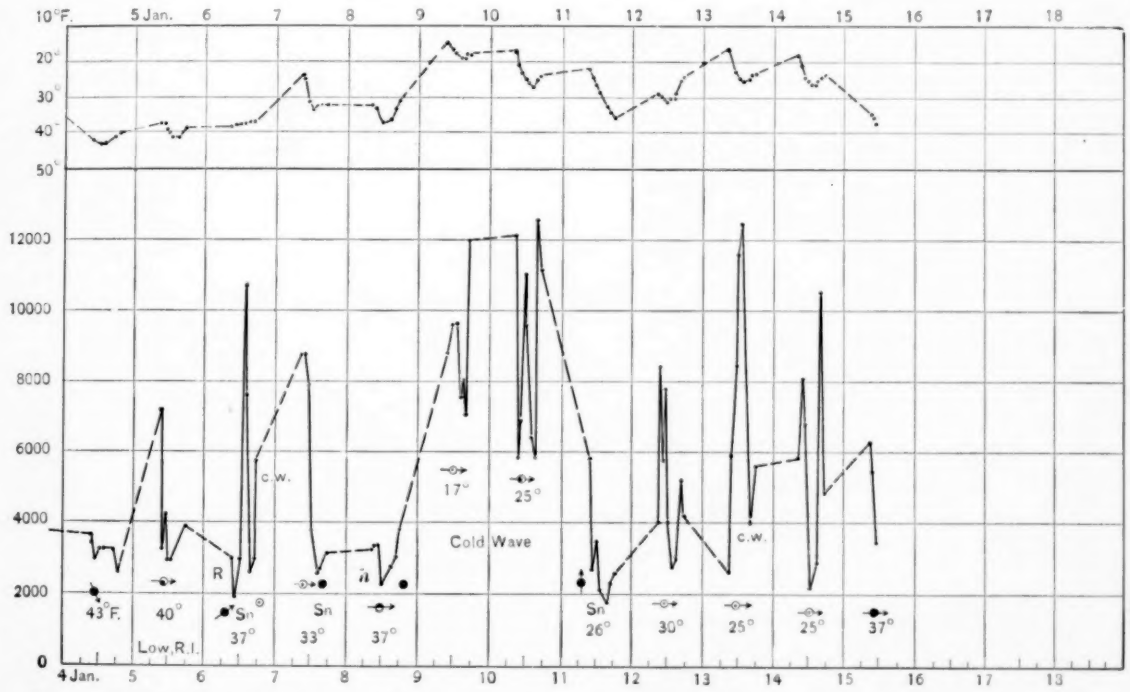


Fig. 6. Nucleation, January 4 to 16, lower curve. Local temperatures (degrees F., at about noon) accompany the curve. Upper curve, corresponding observed temperatures in degrees F.

half a meter of the condensation chamber; but the maximum of atmospheric nucleation does not exhaust the limit of measureable coronas. Nevertheless there are some serious outstanding difficulties: Thus the influx tube must be long (coil and water-bath) to heat the cold air to the given temperature of observation, but if so there may be excessive absorption on the way. It is necessary to wait before observing to insure saturation of the dry winter air (and this is the most insidious of errors, for deficient saturation would be interpreted as high nucleation), but on waiting the time losses of nuclei are increasingly important. All the difficulties are accentuated for high nucleation. Observations carried through long intervals of time, systematically, are alone adapted to test the ultimate trustworthiness of the method and progress is thus necessarily slow.

10. The second general result is the frequent occurrence of maxima of nucleation contemporaneously with sudden fall of atmospheric temperature, often associated with northerly (perhaps overland and dry) winds. When the drop of temperature is alone sufficient, one would have the conditions for the formation and the growth of water nuclei, remembering that persistent nuclei may be made in an atmosphere not too far from saturation, from solutions or in the presence of a solute. Without actual *accessions* to their numbers, however, there could be no increment of the number of nuclei in the condensation chamber prior to the observations at about 20° C. In other words an actual rise and fall of the number of nuclei per c.c. must occur in the atmosphere at the place of observation.

11. As to the wind or convection effect, it is probable that a polar air current or a current from the upper atmosphere will be sometimes associated with the cold waves in question. In this case a part of the region where there is continual production of nuclei has been bodily transported to the place of observation. For the same reason cold air may be nearly free from nuclei.

12. Supposing that the nuclei of any kind are loaded with water by the fall of temperature, various inferences may be drawn to account for the observed increments of nucleation. In the first place, the result may be an accelerated precipitation of an atmospheric layer of excessive nucleation overlying cities and produced, particularly in winter, by combustion. Such conditions are producible

in the laboratory apparatus; for instance I have frequently obtained layers of nucleated air above, below, or even between strata of dust-free air. Before condensation these layers are equally clear; but they are differentiated nevertheless by density. Rise of temperature would retard the precipitation by partly removing the loads of the water nuclei. To test this question as a whole corroborative observations must be made in a place remote from civilization.

13. In the second place if water nuclei newly produced by fall of temperature are radio-active, these would in their turn produce fresh nuclei in succession, and an actual augmentation of nuclei should then be observable, such as has been found in the cold wave maxima or reciprocally in the day minima. In this case the conditions of high nucleation must be shown to correspond to conditions specially favorable to radio-activity, for which there is no evidence. All such questions, however, should find an answer in the direct experiments which I am now making.

BROWN UNIVERSITY, PROVIDENCE.

NOTES ON THE COHERER.

By A. H. Taylor.

Introductory.

THE present knowledge of the action of an electrical contact under varying currents and different distances between the parts of the contacts is limited, and the best coherer theories yet advanced fail to account for the action of this simple yet wonderfully sensitive little instrument as it is actually used in detecting electrical waves or impulses.

This article does not claim to add much to that which is already known on this subject, nor to put forth a complete theory of the coherer. Its object is to give the results of several years' intermittent work with the coherer, coupled with some practical experience with the instrument as used in wireless telegraphy, and to reconcile if possible, these results with the work of others preceding, notably that of Guthe and Trowbridge (Physical Review, July, 1900). The latter authors, as far as I know, are the only ones to give anything like a regular and systematic set of results, represented by curves, or at least, capable of being so represented. Hence their results are the only ones with which it is possible to make much of a comparison.

The use of an ordinary relay as a wireless receiver, using the secondary contact as a coherer, was suggested by the work of Guthe and Trowbridge, but inasmuch as their theory did not explain the action of this instrument as I used it, several curves were taken from it, which were radically different from their curves, although obtained by somewhat similar means. (See Physical Review, July, 1902.) It is also the object of this investigation to verify these new curves.

It appears from previous investigations that very few people have any idea of how sensitive a coherer can be. For instance, a single-contact coherer which I used to get signals from the college to

Lansing, a distance of nearly four miles, would respond at that distance without tuning or synchronizing of the circuits, to a spark of three-eighth inch. The same coherer in combination with a telephone (PHYSICAL REVIEW, July, 1902) would detect the spark from the ignition choke coil of the gasoline motor of an automobile over half a block away. This was no accident, but occurred every time a gasoline-driven machine passed in the street nearest. The effect is a faint but regular clicking in the telephone. The slightest mechanical jar will affect a sensitive coherer, and I hope to show further on in this paper that a displacement of a ten-millionth part of an inch, of the contacts, can not only be observed, but can be *measured* with considerable accuracy. Sparks from a street car trolley nearer than one quarter mile invariably affect the instrument from which most of the data here given were taken. In this laboratory there is a system of electrical clocks, operated from a relay in one master clock, which is about ten meters from the place where most of the observations were taken. It was found absolutely impossible to get any results with these clocks going. Thinking it was the spark from the secondary of the master clock relay, which was in series with the other clocks and four volts, I opened that circuit. The disturbance was less than at first, but still persisted. I then opened the primary circuit of the clock relay, which was in series with 1.5 volts. The disturbance ceased. To verify this, a relay and a gravity cell were set up with a key in series, the key at a distance of about six meters from the coherer. The effect was unmistakable, both on closing and opening the relay circuit. In fact, the coherer seems to be affected by *any* electrical displacement in its neighborhood, but more especially by sparks and electrical displacements in circuits containing self-induction.

Many of the following observations were taken by gas light, as the turning on or off of an electric light in the same room with the coherer often affected it. Lights on A. C. circuits did not affect it nearly as much, and hence were relied upon for light in most cases.

I have found it absolutely impossible to get any satisfactory results in the daytime, and the data to which I attach the most importance were obtained after midnight, when the electric cars had ceased running, and the campus was quiet. It was always found

necessary to stop the clocks and shut off the steam in order to avoid the "thumping" in the pipes. Many a set of observations has been abruptly terminated by the passage of a wagon on a road fully two blocks away. These precautions are not ultra-refinement; they are absolutely necessary if we are to investigate the coherer *in its most sensitive stage.*

From this brief review of some of the difficulties, it can be inferred that a complete solution of the coherer problem may not be forthcoming until the experimenter is able to isolate himself and his apparatus in some remote country where there are no street cars, no electrical storms or such disturbances, no sounds, and no mechanical jars of any kind. Even then he would be bothered by electrical displacements in his measuring apparatus.

For instance, I have found it necessary to put a high resistance in series with the condenser, which, according to the method of Guthe and Trowbridge, is put across the coherer terminals and then discharged through a galvanometer to obtain the terminal coherer potential. This high resistance prevents too sudden fluctuation of the potential at the coherer terminals. Sudden changes of the current through it are sure to affect the state of a coherer when in the sensitive condition.

Considering these difficulties, it is no wonder that it has so far been impossible to get a curve which shall represent the behavior of the coherer in all its stages. The best we can do is to collect a large amount of evidence, and to plot the curves separately for the different stages, and to determine under what conditions one type of curve will predominate, and under what conditions another form will always appear. I have enumerated these difficulties chiefly as a plea for leniency in judgment of results, some of which have undoubtedly experimental errors of as much as 15 per cent.

The tables and curves have been taken from my note-book just as they came, with very little regard to their bearing upon the points which I wish to emphasize. Some of the curves, such as nos. VIII., IX. and XVIII., I would have preferred to omit.

Considerable experience with single-contact coherers in wireless telegraphy practice has demonstrated that such a coherer may be adjusted so as to receive a large nnmber of signals, extending over

even several hours' time, without the use of any decohering device. In other words, the so-called negative coherer action must take place very nearly as often as the positive, when the coherer is in this stage. It has always seemed to me that this stage is also the one of greatest sensibility, and for all wireless telegraphy tests exceeding a half mile in distance, without synchronized circuits, I have found it necessary to adjust the coherer to this stage in order to get the signals. This sensitive stage corresponds to a rather high resistance, a small applied E.M.F and a current varying from 5 to 100 ten-thousandths of an ampère.

This, then, is the stage in which the coherer must be investigated in order to have results applicable to wireless telegraphy practice, and it is in this field that, so far as I am aware, no previous observations have been made.

Three coherers were used during the investigation. The first was the one described in the PHYSICAL REVIEW of July, 1902, and was discarded on account of the difficulty of adjustment for experimental purposes, although for practical work it is the best. The second was the "spherometer coherer." A small piece of nickel was stepped into the foot of the spherometer, which was mounted firmly on a metal base, with an insulated terminal in the center, under the foot of the spherometer screen. This terminal was also of nickel. This coherer had a contact adjustment of .001 mm. and gave quite satisfactory results, except the contact adjustment was was not nearly fine enough.

The third coherer was tho one shown in Fig. 2. The dimensions given in Fig. 3[1] give a nominal contact adjustment of $\frac{1}{6800}$ inch, but the real adjustment is less on account of some unavoidable play in the micrometer screen.

This coherer has the slot, point and plane support for the part carrying the movable contact, and the whole is very heavy and massive to give greater stability. I am greatly indebted to Mr. F. K. Brainard, of the senior mechanical class, for the design and construction of this instrument.

[1] Subcequent measruement and recalculation of this constant gave $\frac{1}{6800}$, which makes the deflection of the contacts by a load of one pound in the middle 1.003×10^{-6}, a value within 0 4 per cent. of the theoretical value. The dimensions given in Fig. 2 are correct.

Not realizing at the outset that changes in distance of the order of the hundred-millionth of an inch between contacts would noticeably affect the coherer, many of the earlier results probably contain very large temperature errors. In later observations the coherer was protected from temperature changes as far as possible.

The ammeter used in the experiments was a Jewel mil-ammeter, calibrated to ten-thousandths of an ampère, having a resistance of 27 ohms and reading to .0150 A. When used with a shunt it read to 1.50 A. and had a resistance of about 0.27 ohm. This instrument was compared with a standard Weston voltmeter and I am certain that the readings are accurate within 2 per cent.

The resistance box was a 10,000-ohm Willyoung box reading to 0.5 ohm and certified to one half of 1 per cent. The galvanometer was a Rowland and was used ballistically with a condenser, and also shunted and in series with 36,000 ohms, with which latter arrangement the coherer terminal potential could be measured directly without drawing enough current to affect the ammeter. This latter arrangement has the advantage of not causing sudden electric displacements in the measuring circuit, as the galvanometer key may be kept closed.

It is logical to suppose that the apparent resistance of a coherer depends upon the distance between the contacts, but it might not be immediately inferred that this apparent resistance depended upon the value of the current passing through it. Inasmuch as no one has, as far as I am aware, obtained a straight line curve of terminal voltage and current, except when the contacts are pressed tight together and have a very low resistance, it is certain that the apparent resistance is variable with the current. The term "apparent" resistance is used here because it is possible that a coherer may show something similar to the back E.M.F. of an electrolyte.

When a coherer is placed in series with a low resistance ammeter and a fixed applied voltage such as a storage cell, a condition of unstable equilibrium results, unless the coherer is *not* in the sensitive stage. I have never been able to adjust a coherer to this extremely sensitive stage with an applied voltage exceeding a few volts, probably not with over four, even with a large "ballasting" resistance in series, and have found it best in wireless telegraphy

practice as well as in these tests to use pressures under two volts. Even with low voltage, but no series resistance, it is impossible to obtain the sensitive adjustment and keep it long enough to take any observations. The current "creeps" up with varying rapidity, and when the creeping has ceased the current has reached a value such that the coherer is no longer extremely sensitive.

For the sensitive adjustment it is necessary to have a low applied voltage, and for the current to be very small in proportion to the coherer terminal potential. There must also be a series resistance, or "ballast." These facts have an important bearing on the theory of the coherer.

When in the sensitive state, the current through the coherer should fluctuate up or down by at least 10 per cent. or 15 per cent., upon the breaking of an inductive circuit such as I used for testing for sensitiveness. This consisted of a 180-ohm relay in series with two volts, the break in the circuit being at least two meters away.

Many attempts were made to shield the coherer from such disturbances as the above-mentioned, but with little success. With the coherer resting on a great block of iron, an old engine base, and completely covered with copper, the disturbing effect of the clock relay was very noticeable. This is undoubtedly due to electrical impulses collected by the lead wires from the testing circuits. Of course the observer must use the utmost precaution against mechanical jars. His chair must not squeak, and if it is necessary to repeat the observations to a second person, it had best be done in a whisper, as the rough guttural tones often affect the coherer if the speaker is close to it. I have found it best to set the coherer on a solid support with a pad of cotton under it. The leads to the coherer can be made of very light wire and fastened firmly to some points on the support before reaching the coherer. This checks the transmission of vibration along the lead wires to the coherer, an effect very difficult to get rid of at times. Many of the observations with the spherometer coherer were made with the instrument in a sling constructed of rubber bands. These absorb the vibrations to a large extent, but naturally it is difficult to adjust the instrument in this dangling position.

Fig. 1 shows the method of connection as used for most of the observations. B is a gravity cell or a chloride accumulator, in series with a small resistance R and a Wheatstone's rheostat, an arrangement allowing a smooth variation of the applied voltage within considerable limits, say from .05 V. to 1.8 V. A is the milammeter, in series with the coherer, CO, and the resistance box, R. The condenser C is connected by means of the charge and discharge key K to the coherer terminals or to the galvanometer G.

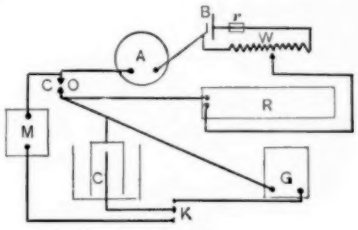


Fig. 1.

The large resistance M (36,000 ohms) is put in series with the condenser and the coherer in order to avoid sudden fluctuations of potential. The current is varied by varying the resistance R and sometimes by varying the applied potential. An ordinary resistance box in the place of R has the great disadvantage of not allowing a smooth change of resistance. On this account several liquid resistances in long tubes, several tubes of varying diameters being in parallel, were tried. This was not, however, as successful as it might have been had the electrodes been larger. The accumulation of gases around the electrodes, and varying polarization made it very difficult to maintain a constant current long enough to take measurements. A large "ballasting" resistance in R and a variable applied voltage seemed to give the best results, the Wheatstone's rheostat giving quite a smooth variation of applied voltage, after the wire and contacts in the rheostat had been polished.

The contacts of the coherer were always nickel. They were cleaned by scraping with a clean steel knife-blade, no attempt being made to preserve any particular contour of the surfaces. Previous experience with the coherer seemed to show that the contact distances were almost of molecular dimensions; hence it is useless to attempt to work with spherical or otherwise particularly shaped surfaces. The most accurately made spherical surfaces which I could have obtained would certainly not have been "smooth" if distances of an order less than the ten-millionth part of an inch are considered. Subsequent investigation showed that the coherer

shown in Fig. 3 was sensitive to such distances. Hence it seems useless to attempt to get smooth contacts. This point is perhaps open to some question, however, and it is possible that polished metal may behave somewhat differently. But contacts prepared simply by scraping with a steel knife-edge are well suited for practical use in wireless telegraphy and it is therefore proper that such contacts should be used here.

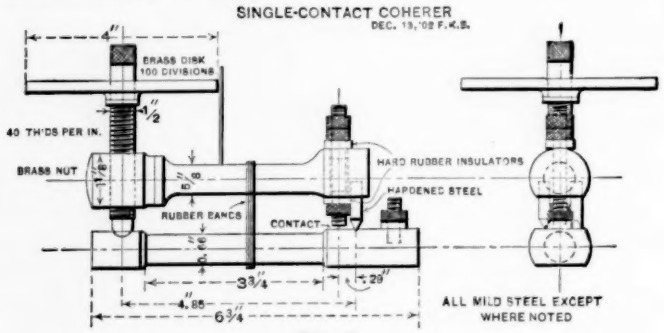


Fig. 2.

The coherer was adjusted to the sensitive condition by changing the distance between the contacts by turning the micrometer head

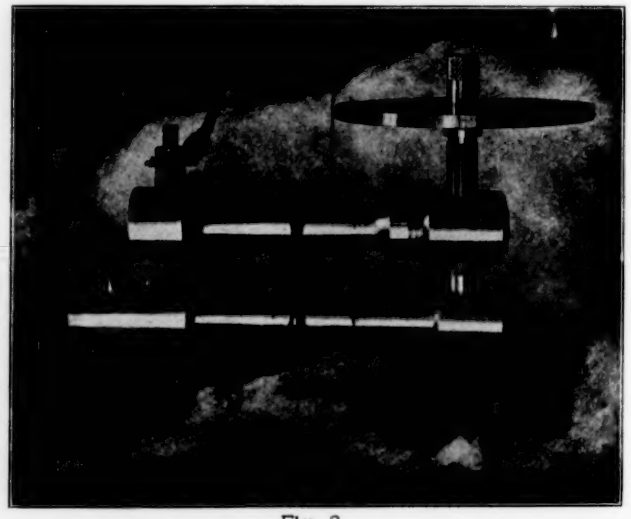


Fig. 3.

(see Fig. 3). One division on the micrometer head should give a charge in contact distances of $\frac{1}{6800}$ inch according to the dimen-

sions given in Fig. 2. This adjustment was very difficult to make, and was best accomplished by regulating the applied voltage and "ballasting" resistance R, until the coherer on short circuit carried from .0007 to .01 ampère. The micrometer head was then adjusted until the current was about 30 per cent. of this value.

This current value was noted, and the coherer tested by a spark not exceeding one millimeter in length, at a distance of about ten meters, or by the opening of the relay circuit previously mentioned. If the current plainly showed fluctuations on thus testing, the coherer was considered to have been in a sensitive condition, and was re-adjusted to the current value just before the test. The best general rule to follow is that the current should be a small part of that which the coherer showed on short-circuiting it.

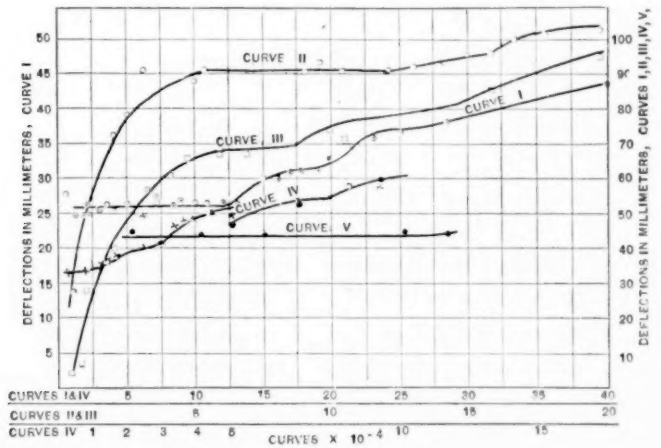


Fig. 5.

In Table I. are given the results of a set of observations for clean contacts in air. The current is expressed in ten-thousandths of an ampère, and the deflections, given in centimeters, are proportional to terminal coherer potential. The constant K was determined by use of a standard cell with the condenser. Curve I. is shown in Fig. 5, and has some striking peculiarities. In the first place there is a long interval, from $C = 1.2$ to $C = 13.0$, during which the potential at the coherer terminals is fairly constant. After this there are several "steps" in the curve, the potential rising rapidly for a little increase in current, and then remaining approximately

constant for an interval. It was thought at first that these steps were due to errors in observation, but inasmuch as they appear in every curve of this type, that is in every curve having intervals of nearly constant potential with increasing current, it was finally concluded that the "steps" were really part of the true curve.

TABLE I.

Capacity 0.5 *mf.* $K = 4.54 \frac{cm.}{volt.}$ *Clean contacts in air.*

$C \times 10^{-4}$	Galv.	Defl.	$C \times 10^{-4}$	Galv.	Defl.
0.5	27.8	2.8	11.0	27.5	2.5
1.2	27.5	2.5	12.2	27.7	2.7
1.5	27.5	2.5	13.0	27.7	2.7
2.0	27.5	2.5	15.0	28.0	3.0
3.0	27.55	2.55	16	28.0	3.0
3.5	27.6	2.6	17	28.1	3.1
4.0	27.55	2.55	18	28.15	3.15
5.0	27.65	2.65	19	28.1	3.1
5.5	28.0	3.0	20	28.3	3.3
6.5	27.8	2.8	21	28.55	3.55
7.0	27.75	2.75	23	28.55	3.55
8.5	27.6	2.6	25	28.65	3.65
9.0	27.65	2.65	28	28.8	3.8
10.0	27.6	2.6	41	29.4	4.4
10.8	27.6	2.6	78	27.5	2.5

TABLE II.

Capacity 0.5. *Clean contacts in Kerosene.* $K = 4.54 \frac{cm.}{volt.}$

$C \times 10^{-4}$	Galv.	Defl.	$C \times 10^{-4}$	Galv.	Defl.
0.5	27.7	2.7	12	34.0	9.0
0.8	30.0	5.0	13	34.2	9.2
1.0	30.2	5.2	14	34.4	9.4
2.0	32.1	7.1	15	34.5	9.5
2.5	32.7	7.7	16	34.6	9.6
2.7	34.0	9.0	16.5	35.0	10.0
5.0	33.8	8.8	18	35.1	10.1
5.5	34.0	9.0	19	35.0	10.0
7.0	34.1	9.1	20	35.23	10.23
9.0	34.0	9.0	21	35.3	10.3
9.5	34.15	9.15	20	36.0	11.0
10.5	34.0	9.0			

In Table II. are given the results for clean contacts in kerosene. Curve II. is similar to Curve I. except that there is at the beginning, between $C = 0.5$ and $C = 5.5$, a gradual rise in voltage. Curve I. is very nearly parallel to the axis of current between the same limits. This preliminary rise of potential before the constant potential and "steps" is shown by other curves to be characteristic of contacts not cleaned before using. Hence it may be that the impurities in the kerosene caused this effect.

Curve II. however, also shows the "steps," there being one more "step" than is shown on the plot. At $C = 21$ occurred a mechanical jar which sent the current back to 20 and raised the potential to 11.0 from 10.3. Such accidents are sure to interrupt the observer before many observations are taken. Curve III. is the only one for clean contacts in air which shows the gradual rise in potential. It differs from curves of the type obtained by Guthe and Trowbridge in that the potential does not reach a finite maximum. If more observations had been taken between $C = 7$ and $C = 20$, this curve might also have shown the steps. This set of observations was terminated by some mechanical disturbance.

Value of K for each Curve.

No. Curve.	K in $\frac{\text{cm.}}{\text{volt.}}$	No. Curve.	K in $\frac{\text{cm.}}{\text{volt.}}$
III	8.17	XII	1.41
IV	1.816	XIII	11.28
V	.908	XIV	1.41
VI	9.09	XV	9.09
VII	9.09	XVI	1.41
VIII	9.09	XVII	4.54
IX	9.09	XVIII	4.54
X	9.09	XIX	2.08
XI	1.41		

Curve.	$K \times 10^{-8}$ $\frac{\text{inches.}}{\text{second.}}$	Curve.	$K \times 10^{-8}$ $\frac{\text{inches.}}{\text{second.}}$
XXI	2.2	XXIV	2.2
XXII	2.2	XXV	2.2
XXIII	2.2	XXVI	2.2

The general form of Curve III. is similar to that of Curve II., and in view of subsequent results it seems more than likely that the con-

tacts were not thoroughly cleaned. Curve IV. is plotted in two separate parts, as a disturbance caused the current to fluctuate uncertainly around the value $C = 5$, and a readjustment was necessary. Both parts of the curve show the steps. Curve V. is rather remarkable in showing an almost constant potential from $C = 5$ to $C = 28$, after which the current suddenly rose to 71 and there was a corresponding drop in potential.

None of these curves is drawn through the origin, as it is by no means certain that they should not intersect the axis of potential difference at a point corresponding to the voltage necessary to start ions across the narrowest part of the gap of the coherer.

Some experiments were undertaken with a view to determining this point. The coherer was placed in series with a Rowland galvanometer sensitive to 1×10^{-7} ampère and a variable source of E.M.F. As the contacts were approached, a gradual creeping of the galvanometer occurred, which corresponded to a leakage current due very likely to the presence of water vapor. Then came a sudden deflection of considerable magnitude, indicating a current of

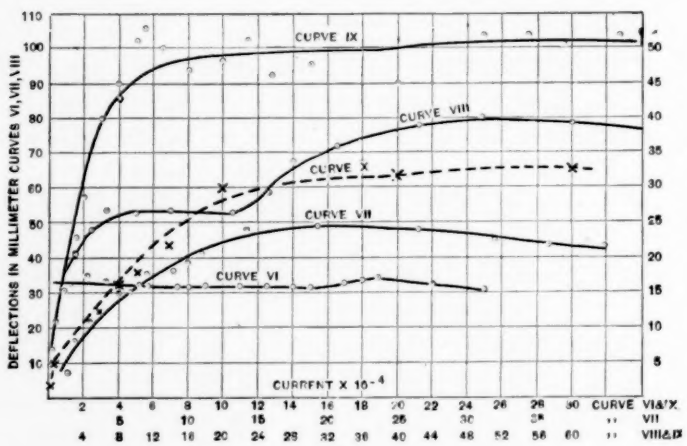


Fig. 6.

several millionths of an ampère. This latter would seem to indicate that the curves should intersect the potential axis. These tests were made with a nominal coherer adjustment of about $\frac{1}{40000}$ inch, which is not nearly sensitive enough. Hence I consider this piece of evidence as unsatisfactory, and the tests will be repeated with the

coherer as described in the latter part of this paper, using an actual adjustment of less than $\frac{1}{10000000}$ inch.

Curve VI. is for clean contacts in kerosene, and shows one step. The potential is nearly constant from $C = 1.2$ to $C = 15$. Compare with this Curve VII. which is for contacts in kerosene after two days' standing in air. Curve VII. is plainly of the same type as those of Guthe and Trowbridge. Curve IX. is also for contacts in kerosene, after three days in air, and is of the same type as Curve VII. Curve X. is also of this type, being for contacts in kerosene after four days in air. The idea in using kerosene was to determine whether the nature of the dielectric affected the type of curve obtained. As far as I am able to judge from these results, the same types of curves are obtained in kerosene as in air.

Curve VIII. is either a mixture of the two types of curves, due to partial oxidation, or it is a magnified "step." I think the first hypothesis the more probable.

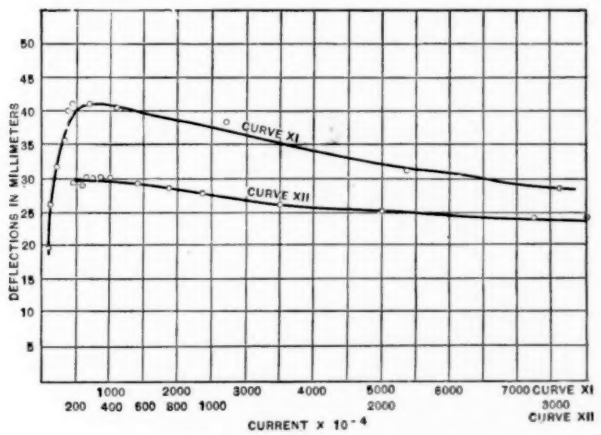


Fig. 7.

Curves XI. and XII. are taken for vastly larger current values than any preceding but plainly show the two types. Curve XII. is for clean contacts in air. Curve XI. is also for clean contacts, but is taken after passing a current of 0.5 ampère through the contacts. The potential drops off very decidedly after reaching a maximum. This is probably a heat effect. The large currents here used develop heat enough to expand the contact terminals and bring them closer

together. Hence the drop in potential. It is not to be understood that curves of the type of Curve XII. are simply parts of curves of the other type. For points on the flat part of curves of the type of Curve XI. the coherer is not sensitive; but is very sensitive for any part of curves like Curve XII. Moreover, repeated trials failed to detect any gradual rest in potential for curves of the latter type. It must be admitted, however, that unless great precautions are taken, the result of the observations will be a curve of the type of Curves XI., VIII., IX. and X. This is due to the fact that the coherer is thrown out of the sensitive state by unclean contacts, by mechanical or electrical disturbances, or by a sudden rush of current, as for Curve XI.

I have never had to use any great precaution in obtaining a curve of the Curve XI. variety, as the behavior of the coherer is not nearly so erratic as when in the very sensitive condition.

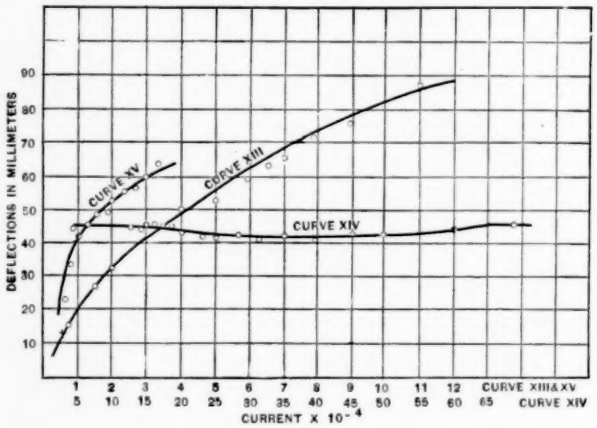


Fig. 8.

Curve XIV. is for clean contacts in kerosene, and shows very nearly constant potential throughout, from $C = 7$ to $C = 68$. There is a slight droop in the middle which may be a temperature effect. Curve XIII. is for contacts which stood for two days in kerosene, while for Curve XV. the coherer was thrown out of the extremely sensitive condition by a 0.5-mm. spark at a distance of 10 meters. The coherer was still fairly sensitive, however, for the conditions represented by Curves XIII. and XV., as I was unable to obtain complete curves on account of disturbances.

Curves XVI. and XVII. are also representative of the two types. Curve XVI. is for clean contacts in air; Curve XVII. is for the same, but affected by 0.5-mm. spark at 10 meters distance.

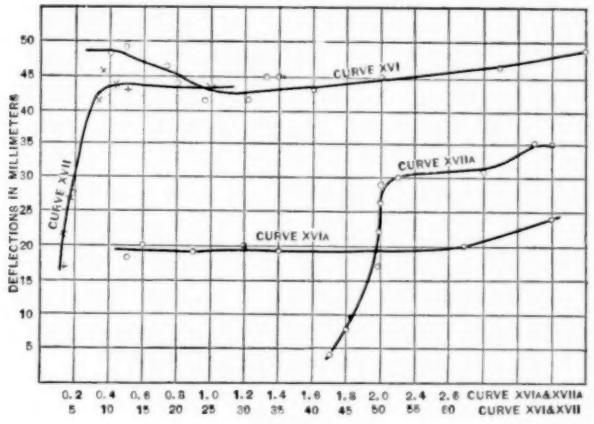


Fig. 9.

Curves XVI_a. and $XVII_a$. are similar curves taken for exceedingly small currents. They are not very reliable on account of the chances for error in estimating the tenths of a division on the ammeter.

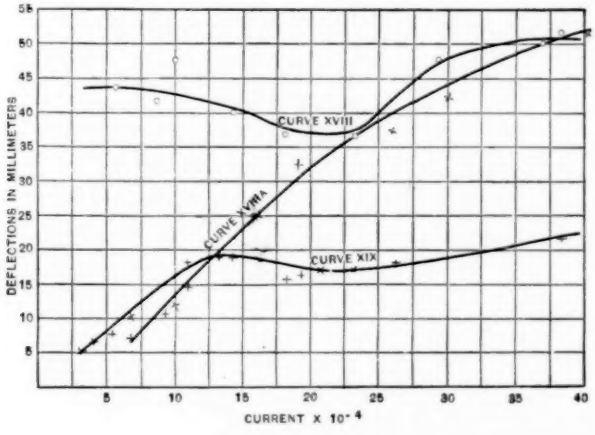


Fig. 10.

Curve XVIII. was taken from the relay coherer, and is rather irregular, showing, however, two steps and a droop in the middle. Curve $XVIII_a$. was taken from the same instrument after being in-

fluenced by a spark at 10 meters. Curve XIX. is taken from the heavy coherer illustrated in this article, and after influence by a spark. It is not very regular, as it was taken in the daytime under rather unfavorable conditions. But irregular as they are, Curves XVIII. and XIX. belong plainly to two different types. Let us call the curves of gradual rise in potential, type *A*, and the curves beginning with periods of constant potential type *B*. The results may be summarized as follows: Type *A* curves are obtained invariably when a high applied potential is used, or when the ballasting resistance *R* (see Fig. 1) is not used, and also when the coherer has been previously influenced by a comparatively large current. Type *A* curves are generally obtained after oxidation, more especially after oxidation for several days, and after influence by a spark from an inductive circuit. Type *A* curves will be obtained whenever the current is large in proportion to the potential at the coherer terminals, which means whenever the contacts are too close together, if we suppose the terminal potential to increase with the distance.

Type *B* curves will be obtained only under the most favorable conditions, with clean contacts, absence of outside disturbances, smoothly varying current and small current in proportion to the total at short circuit. They vary in the extent and number of "steps." This is hardly a fair distinction, however, as no one of the curves is complete, having been cut short by unavoidable disturbances.

Curves of type *A* differ in the slope of the latter part. For large currents the potential decreases (due to heat effect, probably); for medium currents the potential may be slightly rising or falling; for small currents the general tendency is toward a rising potential.

Curves of type *A* would appear to pass through the origin, while those of type *B* would appear to cut the axis of potential, although the evidence for the latter point is weak.

While in the sensitive condition it appears that the conductivity of the coherer varies directly as the current for comparatively great ranges of current. It can be inferred that a curve of type *B* would ultimately end in one of type *A*, on account of the increased applied E.M.F. and increased current. Curves of type *A* are almost always found when the current exceeds a few hundredths of an

ampère. In fact, I have obtained a curve of type *A*, starting from the sensitive condition and a type *B* curve, but owing to unavoidable disturbances in so long a series of observations, there are many interruptions in the curve. Hence I have not given the results. Curve XI. illustrates the point well enough.

These results lead to the conclusion that the coherer phenomena may be divided into three classes corresponding to the three stages in the condition of the contacts. First, there is a feeble leakage current across the contacts due to vapor conductivity. Second, the potential rises high enough to cause conduction by metallic ions. During considerable intervals of current the potential remains nearly constant, then rising by steps until the coherer reaches the third stage corresponding to curves of the *A* type. The coherer can, it seems, be brought directly to the third stage by a strong electrical impulse, or by making the initial contact under comparatively high applied potential. Oxidation and weak electrical impulses cause the curves to become like type *A* even for small currents. But in this case the coherer is often still sensitive, and not in the third stage, for the potential may continue constant for intervals, rise, show steps, and finally develop a second branch of the *A* type. It is then in the third stage. I have seen this actuallly happen, though not in steps free from interruption.

It appears that many of the coherer phenomena can be better understood, if not explained, on the simple assumption that the contact surfaces contain in the limit many pairs of elements of surface approximately parallel and differing in the distance between them. If this assumption be allowed, and the contacts are clean, it follows that as the potential gradient is increased, there will be gaseous or leakage conduction across the contacts. If the applied potential be such as to cause even a very feeble spark, or a comparatively sudden rush of current, the coherer passes at once to the third stage. A comparatively strong electrical impulse has the same effect, causing surgings of potential if the coherer resistance is infinite, and surgings of both current and potential if it is finite.

The theory of Guthe and Trowbridge is perhaps the best that can be applied to this third stage of the coherer, and to the curves

after oxidation, but their equations can by no means be applied to a coherer in its extremely sensitive condition; *e. g.*, in the second stage. Not that a coherer does not respond to some electrical impulses even when in the third stage, such as sparks from an induction coil or a Holtz machine. But it will hardly detect such impulses as those caused by the breaking of a 1.5-volt inductive circuit at a distance of several meters.

Upon further increase of potential gradient, conduction by metallic ions will take place, and hence the resistance will be much lowered. The potential difference is then that necessary to ionize at the nearest pair of contact elements.

If there is no resistance in series, the current will at once rush to such a value that the space between the first pair of elements becomes saturated with ions, and the momentum acquired may raise the potential high enough to ionize at the next pairs of surface elements of nearly the same distance apart. The result will be an unstable condition and an undue increase in current which carries the coherer immediately to stage three.

But with a series resistance, a sudden rush of current would cause a sudden drop of potential which would check ionization. Hence the term "ballast" in speaking of the resistance R in Fig. 1. The potential will then remain very nearly constant until the first pair of surface elements is saturated with ions. Increased current can now be brought about only by increased ionic velocity, hence there comes a rise in potential until that necessary to ionize at the next pair of elements is reached. Then will ensue another interval of nearly constant potential, and the same thing will be repeated again. This hypothesis is based on the "steps" in the B type curve. They always start out with a constant potential, but sooner or later come the "steps," unless the observations are terminated by some untimely disturbance.

When all the spaces between elementary surfaces are saturated with ions, the coherer has reached stage three, and there will be a regular rise in potential until the contacts are welded together by the current, or brought firmly into contact by increased electrostatic attraction.[1]

[1] See Guthe and Trowbridge, PHYSICAL REVIEW, July, 1900.

This condition corresponds to the top branch of Curve XI., but not to the similar branches of all the other curves of the type *A*. For in many cases the coherer is still somewhat sensitive, and the curves if continued show rising potential and sometimes "steps," as in Curves VIII. and II. It may be that the influences of oxidation, of electrical impulses, or of comparatively large currents, extends to only a few of the many pairs of surface elements. For instance, suppose only one contact pair of elements to be brought into actual contact by the influence of a small spark at some distance. The resistance of the coherer would remain nearly constant, though still large, for considerable increase of current, and the voltage would rise gradually until ionization took place at the next pair of contact elements, after which the voltage would remain nearly constant for increasing current until that elementary space was saturated with ions.

But the curve would finally have a second branch of the *A* type when the coherer reached the third stage proper, *e. g.*, when all the contact element's spaces were saturated. Curve VIII. illustrates this point best, although other curves of the *A* type show indications of rising potential at the end, indicating that the coherer has not reached the third stage.

Oxidation of the contacts probably would produce little effect until the film of oxide approached in thickness the distance between the two nearest pairs of contact elements which would normally (with clean contacts) be carrying current. When the oxide attained this thickness, it would require actual contact to cause a current to flow, and the coherer would thus be brought at once to a state giving a curve of the *A* type.

The action of a comparatively large current may be such as to form a welding of the contacts, or a metallic chain across an elementary gap may be produced by a sudden rush of current. This would result in a type *A* curve even before the third stage is reached.

The results and conclusions of this investigation lead to some suggestions as to the action of feeble electrical impulses upon the coherer when in the sensitive stage. Suppose the coherer to be in the condition indicated by a point on a *B* type curve on the line of

nearly constant voltage. This corresponds to an unsaturated condition of one or more elementary contact spaces, and the slightest change in voltage will cause a large change in the current. Hence the coherer will show either increase or decrease of resistance for similar impulses, the electrical surgings set up at the contacts being just as liable to cause positive as negative coherer action. The coherer in the second stage, in series with a small E.M.F. and a ballasting resistance, makes an exceedingly sensitive detector, and shows the positive coherer action to occur very nearly as often as the negative. The slight preponderance of the positive action may be due to the fact that some of the impulses especially in continued action, cause the welding effect, or the formation of a metallic bridge as previously referred to. The fact is that the stronger the impulses the more the positive action predominates. Thus the positive action often has a permanent effect. Even the feeblest impulses may have a permanent effect, as the positive or negative action necessarily means a change in the distribution of the ions and of the electrostatic attraction over the surfaces.

If these suggestions have any value, it follows that the more nearly the coherer curve approaches type *B* the more sensitive is the instrument. Hence when used with large currents (see Curves XI. and XII.), or with partially oxidized contacts (see Curve VIII.), the coherer is not so sensitive as when used with small currents and clean contacts. Such has always been my experience.

If curves of the *B* type really represent the behavior of the coherer in the second stage, and if the conclusions drawn from this investigation, with the assumptions and explanations given, are of any value, it is to be inferred that a curve of *distance* and terminal voltage would show different slopes in different parts.

That is, with a constant current, the rate of change of terminal potential with distance would not be constant, but would vary with the number and contour of the pairs of contact elements carrying current.

Experiments with the coherer shown in Figs. 2 and 3 gave some data from which partial curves of distance and terminal potential were plotted. The solid steel bottom piece of the coherer was placed on a piece of plate glass having a hole under the middle of

the coherer beam. Under the ends of the beam were placed two pieces of No. 22 copper wire which flattened out on the application of a heavy load to the beam, giving an even bearing taking up any irregularities in the glass and bearing points of the beam. The coherer was mounted as shown in Fig. 4.

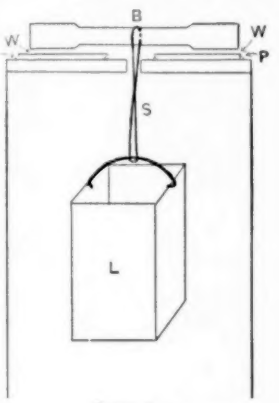


Fig. 4.

A hole was bored in the top of a stout box and two pieces of plate glass, *PP*, placed on the box. At *WW* are the two copper wires, upon which rests the coherer beam *B*. A piece of steel piano wire, *S*, passes over the middle of the beam and supports the load, *L*.

If we assume that the beam bends only in the part of circular cross-section, that is, if we assume the large flat ends of the beam to be rigid, then the dimensions given in Fig. 2 show that the change in the distance of the contacts should be 1×10^{-6} inches per pound load applied at the center of the beam.

In calibrating the beam, the load *L* was a block and tackle with a large spring dynamometer reading to two hundred pounds in 5-pound divisions. The apparatus was connected as in Fig. 1 and the current kept constant at 60×10^{-4} A while the load was varied. The current fell off on increasing the load, and was brought to the desired value by adjusting the micrometer head (see Fig. 3). The calibration was made in terms of divisions on the micrometer head. The curve of calibration is shown in Fig. 2 and from the dimensions given in Fig. 2 and the slope of this curve, we find that one pound applied in the middle of the bar causes a change in contact distance of 0.986×10^{-6} inch, agreeing within 1.4 per cent. with the theoretical constant.

The curve of calibration was very irregular until the copper wires were put in at *WW*, Fig. 4, to take up the unevenness of the bearing surface. Now the coherer is easily sensitive to a load of $\frac{1}{50}$ pound when in the sensitive condition. That is, the ammeter will show a very easily readable deflection on changing the contact distance by two hundred-millionths of an inch, or 2×10^{-8} inch. The ammeter

needle frequently goes clear off the scale for a load of a tenth of a pound; that is a deflection of a ten-millionth of an inch. Breathing on the contacts, or holding the warm hand near them will cause a large deflection, due to different expansion of the nickel contacts and the steel. Placing the finger on the lower beam near the contacts will invariably cause a lowering of the current (due to increased load) and then a rise in the current (due to expansion from the heat of the finger). This shows better than anything else, the wonderful sensitiveness of the single-contact coherer.

It is important that the variation in load be made very uniformly, as otherwise the coherer is liable to be thrown out of the sensitive condition. In this investigation the load was changed by having a syphon empty into or out of the rubber box at L, Fig. 4. This gave a flow of water proportional to time, since the lower arm of the syphon was very long (15 feet) in comparison with the changes in level of the source. The syphon was timed and found to give a fairly regular rate of flow, sufficient to cause a deflection of the contacts of 11×10^{-8} inch for every five seconds of flow. The longest series of observations so far taken, extended over a distance change of only 44×10^{-7} inch, and yet there was a large change of terminal potential within this range.

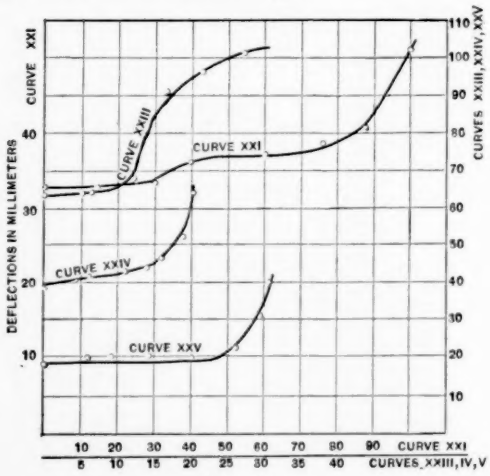

DEFLECTIONS IN MILLIMETERS
CURVE XXI
CURVES XXIII, XXIV, XXV
CURVE XXIII
CURVE XXI
CURVE XXIV
CURVE XXV
10 20 30 40 50 60 70 80 90 CURVE XXI
5 10 15 20 25 30 35 40 CURVES XXIII, IV, V

Fig. 11.

The current was kept constant by means of the Wheatstone's rheostat at W, Fig. 1. Of course with the arrangement shown in

Fig. 4 it is very difficult to protect the apparatus from mechanical disturbances, and the work has to be done after 11:00 P. M. when the electric cars stop running. Under such conditions data are not accumulated very rapidly, and so far I have been able to investigate the variation of terminal potential only with increasing distance. Tables XXI. to XXVI. give the observations taken for distances and terminal potential. The current was kept constant, but at different values for different trials, always being such as to have the coherer in the sensitive condition.

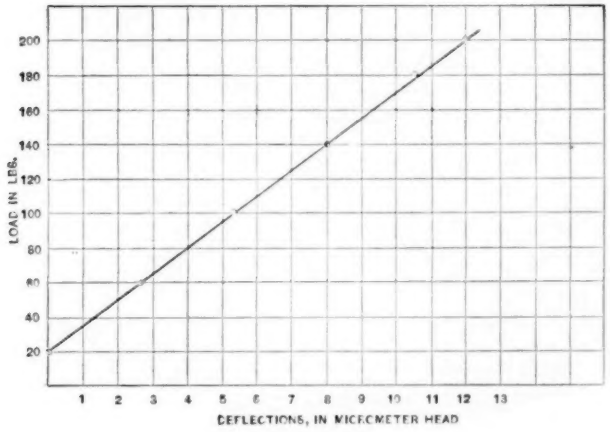


Fig. 12.

Figs. 12 and 13 show the curves of deflection (proportional to terminal voltage) and time (proportional to increase of distance between contacts). By reference to the constant of the syphon K, giving the deflection of the contacts for every second of flow, it is seen that Curves XXII., XXIV. and XXV. cover a range of change of distance of the contacts of only 0.680×10^{-6} inch, and yet show very large changes of terminal potential. Curve XXI. covers a range of 2.2×10^{-6} inch, while Curves XXII. and XXVI. curve about 5.72×10^{-6} inch. I place little reliance upon these curves so far, as they represent observations subject to many experimental errors. However, they are fairly similar, and are in harmony with the conclusions and suggestions already advanced. These observations were terminated in each case by some slight mechanical or electrical disturbances. The coherer seems to be in more and

more unstable equilibrium as the distance between the contacts and the applied voltage increase.

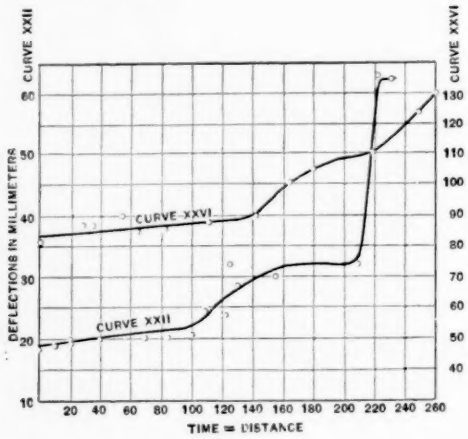


Fig. 13.

GENERAL CONCLUSIONS.

1. The coherer may give either of two distinct types of curves, depending on the applied voltage, the amount of ballasting resistance, the strength of the current, the amount of oxidation of the contacts, and the influence of electrical disturbances.

2. When in a condition such as to give a curve of type *B*, the coherer is most sensitive, and will show either positive or negative coherer action to very nearly the same degree.

3. The coherer is easily sensitive to changes of contact distance of the order of a ten-millionth of an inch, as well as to electrical and mechanical disturbance of exceedingly small magnitude.

4. The order of distance to which the coherer is quite sensitive shows that the *single-contact* coherer really has no practical existence. What we have called a single-contact coherer is in reality a coherer of many contacts.

5. On account of the variable potential, and consequent variable electrostatic attraction, it is not likely that the distance and voltage curves represent very closely the true relations between those two quantities. Still, these curves show considerable similarity, and, in accordance with the assumption of many parallel pairs of contact elements, they show a variable rate of increase of voltage with

distance, the curve getting steeper as the contacts are pulled apart, and fewer contacts come into play.

An interesting method of determining Young's modulus by the use of this apparatus could be devised, and the sample of metal required for such a determination would not need to be very large.

6. The theory of the coherer is far from complete. There are theories which may account for the action of the coherer under special conditions, but there is no theory, as far as I am aware, which satisfactorily explains all the different phases of coherer action. In this paper I have offered some new suggestions, and outlined the explanation of many of the coherer phenomena on the basis of the assumption of parallel elements of contact surface, but I make no attempt to incorporate these ideas into a theory until the problem of the variation of the terminal potential with distance has been further investigated.

In conclusion, let me express my sincere thanks to Professor M. D. Atkins for many valuable suggestions and criticisms made during the progress of the work, and to Mr. F. K. Brainard for assistance in taking observations and making distance tests of the coherers used as wireless telegraphy receivers, as well as for the design and construction of the coherer used for most of these experiments.

PHYSICAL LABORATORY, MICHIGAN AGRICULTURAL COLLEGE,
December 10, 1902.

SOME EXPERIMENTS IN MAGNETIC MOMENT.

BY W. P. BECK.

AT the suggestion of Professor James S. Stevens, and under his direction I have undertaken during the past year a study of the effects of certain mechanical changes upon the magnetic moment of a bar magnet. These changes were comprised under four heads: bending, twisting, concussion, change in temperature. The experiments were carried on in the constant temperature room on the first floor of the Shannon physical laboratory; the conditions for taking observations were favorable, and the liability to errors due to external disturbances was small.

EFFECTS OF BENDING.

A bar of tool steel $1' \times 1'' \times \frac{3}{16}''$ was magnetized by being placed in a helix through which was passing a strong current. It was then placed on two brass blocks as shown in Fig. 1. The edges of the blocks served as knife-edges and were kept at a constant distance of 29.5 cm. apart. The magnetometer was placed on the block *B* and the "end on" position of the magnet bar *l*. Lead weights were placed upon the wooden scale-pan *P*, and resulting changes in deflections of the magnetometer needle were read by a telescope and scale. By means of a micrometer microscope it was seen that the bar returned to its original position when the weights were removed, and therefore the limit of elasticity was not reached in any case.

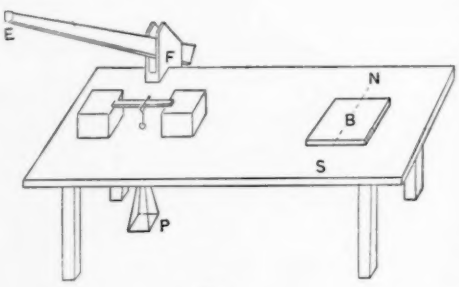


Fig. 1.

TABLE I.

No.	Scale Reading.	Microscope Reading.	Weights on Pan.	Temp. C.
1	46.45	0–2.1	500 g.	17.7°
2	46.54	5–4.2	4,500	17.7
3	46.30	11–9.1	9,650	17.7
4	46.22	14–8.5	12,025	17.7
5	46.24	12–1.2	9,650	17.8
6	46.01	5–8.1	4,500	18.0
7	46.09	0–1.9	500	18.0

For zero deflection, scale reading = 9.84.

Tables II. and III. give representative readings. These were carefully taken but the results are not very satisfactory. In Table IV. a greater bending was effected by use of the lever *EF*. A force due to a weight of 16,000 grams was thus alternately applied and released, with the results indicated in Table IV.

TABLE II.

No.	Total Weight on Bar	Scale Reading.	Temp. C.
1	0 g.	46.51	19.3°
2	590	46.32	19.4
3	2,590	46.14	19.4
4	5,455	46.11	19.4
5	8,500	46.13	19.4
6	10,700	46.08	19.4
7	12,200	45.92	19.4
8	12,200	45.82	19.4
9	15,100	45.52	
10	17,000	44.78	19.0

For zero deflection, scale reading = 9.84.
Distance of S. pole from needle = 76.30 cm.

Curve No. 2 represents the relation between the magnetic moment and the weights applied. The time of taking the observations is noted in the tables, since considerable variations of H occur during 24 hours, and this work assumes H to be constant. The changes in H should be noted here as a source of error, but the general result remains, that bending a magnet tends to decrease its moment.

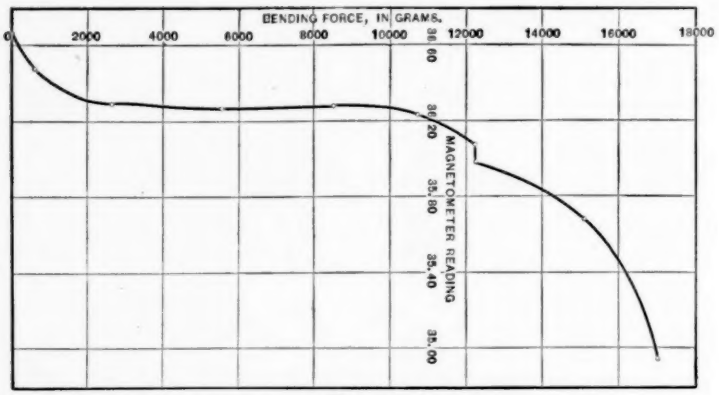


Fig. 2.

TABLE III.

No.	Force, in Grams.	Scale Reading.	Temp. C.
1	0	42.09	18.8°
2	0	42.02	"
3	16,000	41.91	"
4	0	41.81	"
5	16,000	41.77	"
6	0	41.68	"
7	16,000	41.73	"
8	0	41.66	"
9	16,000	41.66	
10	0	41.58	18.8
11	16,000	41.53	"
12	0	41.45	"
13	16,000	41.51	
14	0	41.34	18.8
15	16,000	41.40	
16	0	41.30	18.8
17	0	41.22	
18	0	41.18	
19	0	41.22	
20	16,000	41.28	
21	16,000	41.34	

EFFECTS OF TWISTING.

In order to twist the magnet each end was rigidly fixed in slots in brass blocks, and the center attached to a bar which served as a lever. To the other end of the lever was fastened a cord which in turn was attached to one arm of a balance. By applying weights to the other arm, the desired torsion could be given to the magnet.

TABLE IV.

No.	Force.	Deflection.	Temp. C.
1	0	28.77	17.0°
2	500 g.	28.68	"
3	1,000	28.59	"
4	1,500	28.53	"
5	1,500	28.63	"
6	2,000	28.46	"
7	2,500	28.23	
8	3,000	27.91	17.0
9	3,370	27.66	"
10	3,870	27.19	17.2
11	3,370	27.18	
12	3,000	27.05	17.3
13	2,500	27.04	"
14	2,000	26.90	"
15	1,500	26.76	17.4
16	1,000	26.68	19.2
17	500	26.52	19.1
18	0	26.48	"
19	0	26.25	"
20	500	26.08	
21	1,000	26.06	
22	1,500	25.93	19.9
23	2,000	26.00	"
24	3,000	25.93	
25	3,870	25.78	
26	4,800	25.30	19.9
27	5,670	24.68	

Table IV. expresses the results obtained, and they are shown graphically in plot No. 3.

The results show that there is a loss in magnetic moment produced by twisting; and that the loss is not regained by the magnet when the torsion is removed. As a check the

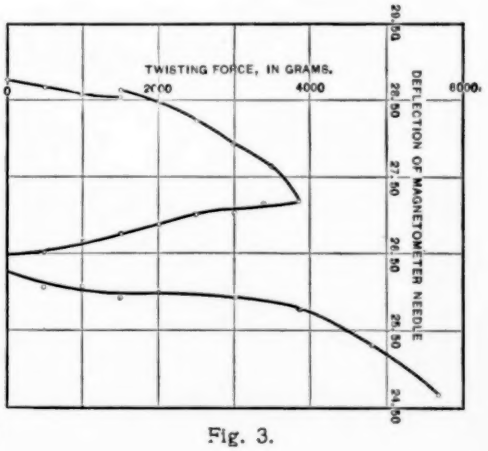


Fig. 3.

magnet was twisted in the opposite direction, when similar results were obtained.

EFFECTS OF CONCUSSION.

The object of this part of the experiment was to discover if any law governed the loss of magnetization when a magnet was severely jarred. The deflection of the magnetometer needle was noted when the magnet was in its normal condition; the magnet was then removed, held vertically five cm. from a stone pier and allowed to drop, striking squarely on its N. pole. It was caught on the rebound and placed in its former position with reference to the magnetometer, and the deflection again read. This was repeated for several heights with results which follow:

TABLE V.

No.	Pole Struck.	Height of Fall.	Scale Reading.	Deflection.	Decrease of Deflection.
1		0 cm.	35.09	20.26	
2	N.	5	34.76	19.93	.33
3	S.	5	34.19	19.36	.57
4	N.	10	33.84	19.01	.35
5	N.	10	33.75	18.92	.09
6	N.	20	33.46	18.63	.29
7	N.	30	31.92	17.09	1.54
8	N.	30	31.81	16.98	.11
9	N.	30	31.67	16.84	.14
10	N.	40	31.78	16.95	−.11
11	N.	50	31.88	17.05	−.10
12	N.	100	28.70	13.87	3.18

No regularity is observed in the falling off of the deflections; but it may be noted that blows of a certain intensity have a greater proportional effect than others, and that if the magnet be struck a series of blows of about the same intensity, the first will produce by far the greatest effect. This would suggest that if one is experimenting with a magnet which he wishes to remain at a constant intensity, but which must undergo some jar in the experiment, it would be well to subject the magnet at the outset to a jar similar to those it is likely to receive.

Conclusion. — In general it may be said that the sources of error which were likely to exist in these measurements were the following: Uncertainty as to values of H at different times; instability of the magnetometer needle; the presence of other bodies in the field which might affect the needle. The first of these has already been discussed. The second was eliminated to a considerable extent, but not entirely. Regarding the third, great pains was taken to make the apparatus free from iron.

SHANNON PHYSICAL LABORATORY, COLBY COLLEGE,
WATERVILLE, ME.

SPECIAL CASES OF THE "VELOCITY OF ENERGY."

BY F. J. ROGERS.

IT is probably true that we cannot, in all cases, individualize a portion of energy — give it "personal identity" — and follow it as it is being transmitted from one place to another. In all cases of wave-motion we naturally assume that the velocity of the energy is the same as the velocity of the waves. In such cases we do individualize a portion of energy and follow it on its journey, namely, the energy associated with a given wave or train of waves.

In 1854 Lord Kelvin[1] made an indirect application of this idea. The final object of the paper referred to was to make an estimate of the density and elasticity of the ether. The immediate object was the determination of the energy of a "cubic mile of sunlight." The method was briefly to assume that all the energy in 186,000 (miles)^3 of sunlight is received in one second by a surface of one square mile taken perpendicular to the sun's rays, and then to compute the energy in one cubic mile from the energy received in one second. This latter can be readily computed from the value of the "solar constant" of approximately 3 calories per cm.^{2} per minute.

The relationship of energy per unit volume, power transmitted and velocity may be expressed in the form of the following equation

$$\frac{E}{V}=\frac{P/a}{v}, \tag{1}$$

in which P/a is the power transmitted per unit area taken at right angles to the direction of transmission, E/V is the energy per unit volume stored in the medium, and v is the velocity of transmission of the energy.

[1] "Note on the Mechanical Value of a Cubic mile of Sunlight." Math. and Phys. Papers, Vol. II.

Now there is nothing to prevent our transforming equation (1) and using it to compute the velocity of energy when the other quantities are known.

$$v = \frac{P/a}{E/V} \tag{2}$$

Let us apply this equation to the computation of the velocity of energy in a few special cases.

VELOCITY OF ENERGY ALONG AN ELECTRICAL CIRCUIT.

For this purpose we shall take the special case of concentric tubular conductors in which the resistance is so small that the fall of potential thereby produced may be neglected. In this case it will be more convenient to put (2) into the form

$$v = \frac{P}{E/l} \tag{3}$$

in which P is the whole power transmitted and E/l is the energy per unit *length* stored in the dielectric between the two conductors.

Using symbols with their usual signification equation (3) becomes for the electrical case

$$v = \frac{(V_1 - V_2)I}{\int \frac{KE^2}{8\pi} dA + \int \frac{\mu H^2}{8\pi} dA}. \tag{4}$$

The two terms in the denominator of (4) represent the electrostatic and the electromagnetic energy respectively and are to be integrated over the cross-section of the dielectric between the conductors.

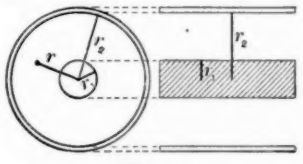


Fig. 1.

In the case of tubular conductors (see Fig. 1) of radii r_1 and r_2 the values of E and H at distance r from the common center may be expressed as follows :

$$E = \frac{V_1 - V_2}{r \log_\varepsilon \frac{r_2}{r_1}}, \quad H = \frac{2I}{r}. \tag{5}$$

Substituting these values in equation (4), integrating over the cross-section of the dielectric we obtain, when K and b are constant,

$$v = \frac{(V_1 - V_2)I}{\frac{K(V_1-V_2)^2}{4 \log \varepsilon \frac{r_2}{r_1}} + \mu I^2 \log \varepsilon \frac{r_2}{r_1}}. \tag{6}$$

Using A and B as constants we may write (6) as follows:

$$v = \frac{(V_1 - V_2)I}{A(V_1 - V_2)^2 + BI^2}. \tag{7}$$

From the inspection of (7) it is obvious that the velocity is zero when either I or $(V_1 - V_2)$ is zero and that it is a maximum when

$$A(V_1 - V_2)^2 = BI^2 \text{ or } \frac{V_1 - V_2}{I} = \sqrt{\frac{B}{A}}. \tag{8}$$

This means that the velocity will be a maximum when the electrostatic energy is equal to the magnetic energy. Substituting in (7) for $(V_1 - V_2)/I$ its value obtained from (8) and we have

$$v_{\max.} = \tfrac{1}{2} \sqrt{\frac{1}{AB}} = \frac{1}{\surd K\mu}. \tag{9}$$

We may arrive at the same result as above by making use of Poynting's theorem. Assume an electromagnetic field in which the electric and magnetic lines of force are mutually perpendicular. For such a case Poynting's theorem gives

$$\frac{P}{a} = \frac{HE}{4\pi}.$$

The energy per unit volume is as before

$$\frac{KE^2}{8\pi} + \frac{\mu H^2}{8\pi}.$$

Substituting in (2), cancelling the 4π, and we obtain

$$v = \frac{HE}{\frac{1}{2}KE^2 + \frac{1}{2}\mu H^2}.$$

The maximum value of this expression is

$$v_{\max.} = \frac{1}{\sqrt{K\mu}}. \tag{10}$$

It may be impossible to form a mental picture of the motion of energy through an electromagnetic field when the latter is perfectly steady and unchanging at every point. Nevertheless, it is interesting to find that the above method gives for the *maximum* velocity with which energy may be transmitted by an electromagnetic field the same value as the velocity of electromagnetic waves.

Velocity of Energy Along a Rotating Shaft.

Let the shaft be of uniform circular section of radius r, moving with the constant angular velocity ω and sustaining a uniform angular twist of φ/l radians per unit length. If G_0 is the torque required to twist one end of the shaft through unit angle when the other end is clamped the power transmitted by the shaft and absorbed at the distant end will be

$$P = G_0\varphi\omega. \tag{11}$$

The energy, both kinetic and elastic, stored in unit length of the shaft is

$$E/l = \tfrac{1}{2}\frac{K\omega^2}{l} + \tfrac{1}{2}\frac{G_0\varphi^2}{l}. \tag{12}$$

Substituting these values in (3) we have

$$v = \frac{2l\,G_0\,\varphi\omega}{K\omega^2 + G_0\varphi^2}. \tag{13}$$

This shows that the velocity with which energy is transmitted along a rotating shaft varies with the relative values of the angular velocity and the angular strain; also that this velocity is a maximum when the kinetic and elastic energies are equal. This maximum velocity is

$$v_{\max.} = l\sqrt{\frac{G_0}{K}}. \tag{14}$$

If G_0 is expressed in terms of the modulus of rigidity N, and dimensions and K in terms of the density ρ, (14) becomes

$$v_{\max.} = \sqrt{\frac{N}{\rho}}.$$

But this is just the velocity with which torsional waves would be transmitted by the shaft.

VELOCITY OF ENERGY TRANSMITTED BY A FLUID UNDER PRESSURE.

In the case of a liquid in which stress is proportional to strain the maximum velocity of energy deduced in a manner similar to the above comes out the same as the velocity of compressional waves.

STANFORD UNIVERSITY,
May, 1902.

PROCEEDINGS

OF THE

AMERICAN PHYSICAL SOCIETY.

MINUTES OF THE NINETEENTH MEETING.

A REGULAR meeting of the American Physical Society was held in Fayerweather Hall, Columbia University, New York City, on Saturday, February 28, 1903, President Webster presiding.

The following papers were presented:

The Nucleation of the Atmosphere During Cold Weather. CARL BARUS.

An Experiment with the Holtz Machine. CARL BARUS.

The Nucleation and Ionization of Phosphorus. CARL BARUS.

Diffusion and Supersaturation. G. W. PIERCE and H. W. MORSE.

The Rôle of Thermal Electromotive Forces in a Voltaic Cell. HENRY S. CARHART.

A Method of Determining Internal Resistance of Cells, especially applicable to those which polarize rapidly. J. R. BENTON.

A Simple Geometrical Principle and its possible Relation to a General Physical Theory. JOHN MILLIS.

Adjourned.

ERNEST MERRITT,
Secretary.

RESULTS OF RECENT COMPARISONS OF MAGNETIC INSTRUMENTS.[1]

BY L. A. BAUER.

THE Coast and Geodetic Survey has been engaged during the past three years in making elaborate comparisons of its various magnetometers and dip circles, almost every known variety of make being represented. The dip instrument selected as standard is a large earth-inductor of the Wild-Edelmann pattern, mounted at the Cheltenham Magnetic Observatory. This superb instrument has been found to agree

[1] Abstract of a paper presented at the meeting of the Physical Society held on December 31, 1902.

with two other inductors, one of Eschenhagen-Schulze design and the other the portable Wild-Edelmann pattern, within 0.1 of a minute of arc. The various dip circles were found to have corrections on this standard amounting in some instances to as much as 6 and 8 minutes of arc; these large corrections are in general traceable to impurities in the brass of which the instruments were constructed. The magnetometers, with the exception of three cases, were found, in general, to be in good accord. The standard is a large Wild-Edelmann observatory magnetometer at the Cheltenham Magnetic Observatory.[1]

Next a *brief statement* was given *of the results of the international magnetic observations made during the total solar eclipse of May 18, 1901.* A small magnetic variation again revealed itself, as during the eclipse in the United States, May 28, 1900, this variation being detected at stations within the belt of totality and at the Batavia magnetic observatory, 350 miles south of the central line of the shadow and at the Manila observatory, three times the distance north of the central line that Batavia was south of it. It was shown that the nature of this variation is precisely similar to that of the solar diurnal variation. We have thus had revealed to us a phenomenon that will play an important part in unfolding the causes of the solar diurnal variation of the earth's magnetism.[2]

NOTE ON A SUBSTITUTE FOR A SMOKE FILM ON GLASS.[3]

BY ERNEST BLAKER.

OVER a year ago it seemed advisable to find some substitute for smoke for coating glass, on which to get tracings of a stylus attached to a prong of a freely falling tuning fork. The fork and its carriage are a part of a piece of apparatus used by students in the Junior Physical Laboratory, in the study of uniformly accelerated motion and in the determination of g. The glass plate on which the tracings are made is about 140 cm. long, 15 cm. wide and 1 cm. thick and to coat it uniformly with smoke film is a matter of some difficulty. Among the substances tried was a soap used to clean windows and of general use in scouring. This soap, Bon Ami, gave the best results and has been used ever since in the experiments above noted.

If the Bon Ami be put on the glass in a thin layer, by using a wet piece of cloth or waste which has been rubbed over the soap and allowed

[1] This paper will be published in the Annual Report of the Superintendent of the Coast and Geodetic Survey.

[2] This paper appears in full in the December, 1902, issue of Terrestrial Magnetism and Atmospheric Electricity.

[3] Abstract of a paper presented at the meeting of the Physical Society held on October 25, 1902.

to dry before using, it will be found that very fine, clean tracings may be obtained. If intersecting lines are desired the intersections will be found to be very sharp, there not being the liability of checking as with smoke when not properly put on.

It is necessary that the stylus be a little stiffer when used with Bon Ami than with a fine smoke film, and when Bon Ami is used in the manner suitable for the above noted experiments the damping may be a little greater than if a stylus suited for smoke be used, as the film is perhaps a little tougher. However, in tests made in which the glass was covered with parallel vertical stripes of soap film and smoke film, the average wave-length under the same conditions of position and distance from the origin was found to be a little greater on the Bon Ami film than on that part of the plate covered by smoke. This would indicate either a greater damping or a greater acceleration on the Bon-Ami-covered portion of the plate.

When tracings are made with a stylus driven by levers or a mechanism not affected by damping the smoke film has no advantage over the Bon Ami.

The Bon Ami tracings on films that are quite thick yield to fixing with shellac as do those on smoke, but such tracings are not good for lantern projection, the film being white and allowing much light to pass through it. For such work it cannot then take the place of smoke films. This is the indication of the observations here.

The Bon Ami film is so tough that a scale may be placed directly on it for measurements without damage, and it does not stick to the scale.

The advantages in its favor for such work as has been noted, over smoke, are its ease of application and consequently lessening of the danger of breaking the glass plates due to handling or to unequal heating; the clearness of the lines traced and the sharpness of intersections; and the ease of removal by simply rubbing off with a dry cloth, which leaves a clean glass surface without scratches.

PHYSICAL LABORATORY OF CORNELL UNIVERSITY.

RADIOACTIVITY FROM FRESHLY FALLEN SNOW.[1]

BY S. J. ALLAN.

C. T. R. WILSON has shown that freshly fallen rain, after being evaporated down to dryness, leaves behind it a radioactivity which decays with time. The author was led to believe that freshly fallen snow would also show this property. This on examination proved to be the case. Freshly fallen snow was gathered from a thin layer on the surface

[1] Abstract of a paper presented at the meeting of the Physical Society held on December 31, 1902.

during a snow storm. This on being evaporated down to dryness in a metal dish, and tested was found to ionize the air in the immediate vicinity. From about .1 liter of snow during a heavy snow storm could be obtained about the same effect as from about .1 gram of uranium. Decay curves were taken and the activity was found to fall off in a regular geometrical progression with time, falling to half value in about 30 minutes. This is quite unlike the excited radioactivity from air as obtained on a negatively charged wire, which decays to half value in about 48 minutes. The penetrating power was found to be about the same as that for the excited activity on a charged wire. Experiments over a wide range showed that the amount of radioactivity in a given quantity of snow varied as the amount of snow falling per second, and was constant so long as the fall of snow was constant. At the end of 24 hours after the snow had ceased to fall only a very minute fraction of the radioactivity remained. The conclusions to be drawn from these results are, that this radioactivity is different from the excited, or that the rate of decay for the excited is more complicated than at first supposed and may have different values in different portions of the curve. These points are at present under investigation.

INDUCED RADIOACTIVITY EXCITED IN AIR AT THE FOOT OF WATERFALLS.[1]

BY J. C. MCLENNAN.

A LITTLE over a year ago Elster and Geitel[2] found that if a negatively electrified wire were exposed for some hours in the open air or in a very large room, it became temporarily radioactive. Since then a number of observations have been made upon this effect, and the consensus of opinion appears to be that it is due to the presence in atmosphere of some peculiar constituent similar to the emanation from thorium, which has been shown by Rutherford[3] to induce radioactivity in any body with which it comes in contact, especially when that body is negatively electrified.

The difficulty of determining and of regulating the atmospheric conditions for observations upon this excited or induced radioactivity suggested the desirability of resorting, for purposes of experiment, to a locality where exceptional electrical conditions were known to exist permanently in the atmosphere. Niagara Falls, according to Lenard's[4]

[1] Abstract of a paper presented at the meeting of the American Physical Society held on December 31, 1902.

[2] Elster and Geitel, Phys. Zeit., No. 40, p. 590, 1901.

[3] E. Rutherford, Phil. Mag., No. 49, pp. 1 and 161, 1900.

[4] Lenard, Wied. Ann., 46, p. 584, 1902.

results, is preëminently such a locality, as the fine drops of spray into which water is broken on passing over waterfalls were found by him to communicate, on striking the wet rocks at the foot of the falls, a negative charge of electricity to the surrounding air and a positive charge to the water. Through the kindness of the Hon. Thomas Walsh, Superintendent of the Niagara Falls National Park Reservation, the author was enabled in September last to carry out a short series of observations upon excited radioactivity at the foot of the falls. The general result of the investigation was that, during the course of the experiments, the amount of radioactivity induced in a wire exposed at the foot of the falls was found to be very much less than that in a wire exposed in the same manner in Toronto.

Apparatus.—In these observations the measurements were made with a quadrant electrometer of the Mascart type as constructed by Carpentier. The silk suspension in the original apparatus was replaced by a phosphor bronze strip less than 0.025 mm. in thickness, which was attached at its upper end to an ebonite rod to secure insulation. The needle was kept charged by a battery of small storage cells similar to those installed in the Reichsanstalt. The deflections were measured by the movement of the image of an incandescent lamp filament upon a transparent scale placed at a distance of one meter from the electrometer.

With a potential of 480 volts applied to the needle, the sensibility of the instrument was such as to produce a deflection of 1,000 mm. on the scale for a potential difference of one volt between the quadrants. In measuring the induced radioactivity bare copper wire, No. 24, was exposed in the open air by means of specially constructed insulating supports, Fig. 1, attached to a series of bamboo poles erected at convenient distances. The wire was charged by a small Toepler-Holtz electrical machine driven by a water motor, which maintained a potential of from eight to ten thousand volts.

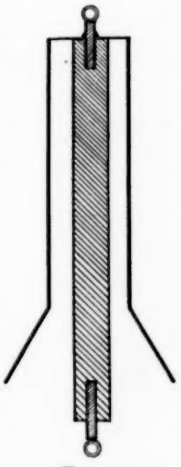


Fig. 1.

The insulator shown in Fig. 1 consisted of a brass tube about 20 centimeters long, closed at one end and having a bell-shaped opening at the other. Into this tube an ebonite rod was screwed, which could be easily removed when it was necessary to renew its surface. Hooks fastened to the tube and to the ebonite rod provided for the support of the insulator and the suspension of the wire.

In the experiments at the foot of the Falls it was found that, with these insulators, a wire could easily be maintained at a potential of 10,000 volts for hours even in a drenching spray.

The exposed wire was tested for induced radioactivity by means of the apparatus shown in Fig. 2. *A* is a galvanized-iron cylinder about 30 cm. high and 20 cm. in diameter, resting on an insulated platform and having a movable cover provided with a flanged opening into which was fitted an ebonite plug about 5 cm. in diameter. A brass tube *C* was passed through this plug and into it a second ebonite plug was tightly fitted. This second plug carried a brass rod *D* from which *B*, a brass reel, was suspended, and on this reel the wire to be tested was wound. The brass tube *C*, which was earthed throughout the measurements, served as a guard ring and prevented any leak from the vessel *A* to the rod *D* across the ebonite plugs.

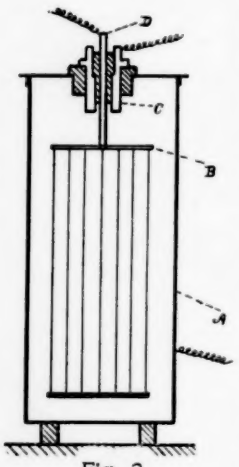


Fig. 2.

The saturation current, due to spontaneous ionization of the air in the vessel, having been first determined by connecting the supporting rod *D* to the electrometer in the usual way, and applying a potential of 100 volts to *A*, the exposed wire was then wound on the reel, inserted in the vessel, and the saturation current again ascertained. Any increase observed in the ionization current was taken as a measure of the radioactivity induced in the exposed wire.

As it was impossible, in setting up the apparatus in two different localities, to be certain that it was of exactly the same sensibility, a standard of ionization was deemed necessary for purposes of comparison. A radioactive substance which the writer had in his laboratory at the time of making the experiments was chosen, and a small quantity in a glass phial being found to give a constant ionization current when placed in a given position in the chamber *A*, this current was adopted as a standard and the ionization currents measured in the various tests were expressed in terms of it.

Experiments. — Both before proceeding to Niagara and after returning a series of exposures was made in the quadrangle of the University of Toronto. Copper wires, approximately 30 m. long, were exposed on a number of days for periods of two hours at a potential of 8,000 to 10,000 volts. After a wire had been exposed it was placed in the ionization chamber, the saturation current measured and its radioactivity, thus ascertained, was expressed in terms of the standard radioactive substance.

The resulting values, taking the standard saturation current as unity, showed considerable variation in the amount of radioactivity excited, the highest value observed being 1.75, and the lowest 0.6. The intermediate values ranged between these limits, but were quite irregular and seemed

to depend more upon the existence of wind and its velocity than other changes in the weather. The occurrence of showers did not exert a noticeable effect on the amount of radioactivity excited. It was frequently observed that exposures made in the morning gave greater induced radioactivity than those in the afternoon, but the exposures made after sunset gave values generally equal to and sometimes greater than those of the morning.

The observations were made in Toronto between September 5 and 12, and between the 19th and 23d, those made at Niagara occupying the interval. The values in the two series at Toronto exhibited similar variations and were confined in both cases within the limits mentioned. The weather throughout, except for a few showers, was uniformly fine.

In conducting the experiments at Niagara Falls the electrometer and its attachments were set up in the basement of the landing station at the foot of the inclined railway. It was a large room, having stone walls and a cement floor, and, being fairly dry, was well suited for making the tests. The electrical machine which was intended for charging the wire was also located in this room, and the wire to be exposed was led from it out of a window and suspended in three stretches of about 30 m. each, the insulation being secured as already described.

The situation was admirably adapted for making the exposures, as the wire could be led within a meter or two of the vast mass of falling water. The first length of wire was generally enveloped in a very fine spray, that which surrounded the second was heavier, while the downpour upon the third resembled the heaviest rain.

A point of peculiar interest in connection with the first exposure was that the wire, upon suspension in the spray, immediately became negatively charged to a potential of about 7,500 volts. This voltage was maintained with but little variation both day and night during the period covered by the experiments and the sign of the electrification was invariably negative.

The results of a number of tests with the different sections of the wire showed that the removal of the third generally caused a drop in the potential of the remaining sections from 7,500 volts to about 5,500 volts, while the first section, when exposed alone, gave a potential varying from 3,000 to 4,000 volts. From this it was evident that the spray was the cause of the electrification and that the potential of the wire was largely determined by its density. On account of the permanency of this negative electrification of the exposed wire, the electrical machine necessary in the Toronto experiments was not employed.

In testing for induced radioactivity, the wire was exposed for periods of about two hours and was removed in sections, each section being replaced by a new length. The same method of testing was used as in

Toronto and the results expressed in terms of the standard radioactive substance.

Exposures were made on four different days with the following results, *A*, *B*, and *C*, denoting the sections of the wire, of which *A* was that nearest the observing station and *C* that nearest the falls.

Date.	Section of Wire Tested.	Induced Radioactivity. Saturation Current of Standard Substance 1.0.
September 12.	*A*	0.1
	B	0.31
	C	None observed.
" 13.	*A*	0.16
	B	None observed.
	C	Not exposed.
" 16.	*A*	0.13
	B	0.14
	C	0.11
" 17.	*A*	0.15
	B	0.12
	C	None observed.

From these numbers it will be seen that but little variation was observed in the radioactivity excited in the sections *A* and *B*. In *C*, however, which was nearest the falls, measureable radioactivity was present in only one of three exposures.

Ordinarily this section was drenched by masses of falling water in addition to being surrounded by fine spray such as enveloped the rest of the wire. But on September 16, the air currents during exposure were such as to drive aside the sheets of water and permit only the fine spray to come in contact with the section.

As already stated the spray enveloping the section *B* was generally heavier than that in contact with *A*, but the condition was not permanent, as the spray was blown about by gusts of wind whose direction was continually changing in the gorge.

The chief interest attaching to the results was that the radioactivity excited was much less than that in Toronto. It will be noticed that the greatest amount of radioactivity observed at Niagara Falls was 0.3, while the least observed in Toronto during the period covered by the experiments was 0.6. The numbers also show that on the average the radioactivity excited at Toronto was at least from six to seven times greater than that induced at the falls.

The observations were not made in the two places at the same time, and a direct comparison is therefore impossible, but, as there was no break

in the weather during the progress of the experiments, and as the values found in Toronto before going to the falls were almost the same as those obtained on returning, one is warranted in concluding that the air at the foot of the falls permanently possesses less power to excite radioactivity than the air in localities at some distance.

While the experiments were in progress the spray was frequently examined for radioactivity. This was done by collecting a quantity, evaporating it and testing the containing vessel. In no case was any trace of radioactivity observed.

UNIVERSITY OF TORONTO,
October 30, 1902.

EXPERIMENTS CONCERNING VERY BRIEF ELECTRICAL CONTACTS.[1]

BY HERSCHEL C. PARKER.

A SERIES of electrical contacts giving a fairly accurate range of adjustment from 0.1 second to 0.00001 second would furnish a valuable means of investigation. A gravity contact key devised by Dr. Charles Forbes gives promise of fulfilling the above conditions. The writer has made many determinations of the times of contact given by the various devices employed on this key, and has also investigated the times of contact of several forms of pendulum. The method employed was as follows: a condenser of known capacity (F, farads) was charged during the time of contact (t) and the deflection on discharging noted. This deflection (if a good mica condenser is used which has no absorption) is proportional to the electromotive force (E) and the capacity (F). The condenser is again charged through a resistance (R) and the deflection (Q) observed.

Then: $Q = EF \times (1 - e^{\frac{t}{RF}})$ and, $t = -RF \times \log e \times \left(1 - \frac{Q}{EF}\right)$.

The method while very easy to apply allows a wide range in the resistances and deflections with but little effect on the values of the time. An example will best show this. 1st, $F = 0.00000048$, $R = 1000$, $EF = 120$, $Q = 43\frac{1}{2}$, $t = 0.00026$ second. 2d, $F = 0.00000048$, $R = 10{,}000$, $EF = 120$, $Q = 5\frac{1}{2}$, $t = 0.00027$ seconds.

The "gravity key" consists essentially of a rectangular weight falling on metal guides, the key being furnished with a scale divided in fractions of a second according to the law of falling bodies, and the weight actuating the various forms of switches employed. If two switches are used,

[1] Abstract of a paper presented at the meeting of the Physical Society held on December 30, 1902.

one to make the contact and the other to break the contact, by placing them at different distances apart on the scale times of contact varying from 0.4 second to 0.001 second may be obtained. For shorter times, a single switch that makes and breaks the contact is made use of and the time made faster or slower by placing in different positions on the scale so that the falling weight strikes it with varying velocities.

In one form, the weight moves the short arm of a lever, the long arm passing over a contact strip. Another form is one in which the fulcrum of the lever changes, first giving contact and then breaking the circuit immediately afterwards. By placing this form of key in different positions on the scale, times of contact varying from 0.0015 second to 0.000035 second may be obtained. In still another type, which seems the most rapid and positive contact of the three, the falling weight striking a lever arm releases a spring which makes the contact and a further motion of the lever breaks the contact, thus giving a differential effect between the velocity of the weight and the rapidity of the spring. With this key, while a contact as slow as 0.02 second can be made, the rapidity of 0.000017 second may be obtained and it seems possible with the most careful adjustment to reach 0.00001 second. The limiting condition in all these keys is the mechanical adjustment, for as the time becomes very short the contact becomes uncertain and may frequently fail together.

Experiments made with pendulums consisting of a steel ball suspended by a wire and striking against a steel anvil, gave very positive and satisfactory contacts. These contacts, while very brief, give only a slight variation in time. Using a pendulum with a suspension wire about 4 meters long and the steel ball 2 inches in diameter, an arc of ½ degree gave 0.00039 second and an arc of 15 degrees, 0.00016 second, while a pendulum with a short suspension wire using a ½-inch steel ball, through an arc of 90 degrees gave 0.000079 second.

It is interesting to note that in working with condensers, the best mica condenser gives no appreciable variation in capacity for practically the briefest times of charge, while a paraffine condenser may show a reduction in capacity of some 60 per cent. from a time of charge of 1 second to that of 0.001 second.

THE CRITICAL CURRENT DENSITY AND THE FALL OF POTENTIAL AT THE CATHODE IN VACUUM TUBES.[1]

BY CLARENCE A. SKINNER.

THIS investigation was undertaken in order to arrive at an explanation for the difference in results obtained by the author for the fall of potential at a disk cathode and those given by Stark for a wire cathode.

[1] Abstract of a paper presented at the meeting of the Physical Society held on January 1, 1903.

It has been repeatedly observed that the discharge from the cathode tends to maintain a certain current density by confining itself to a part of the cathode surface only, if the current drops below a certain value. With increasing current the discharge gradually spreads over the cathode until the exposed surface is wholly covered. Within these limits the cathode fall, as shown by Hittorf, remains constant. H. A. Wilson has shown that the current density also remains constant — equal to about $0.4\,p$ milli-amp./square cm. in air, where p designates the pressure of air in mm. When the cathode is wholly covered by the discharge its fall of potential increases with the current, that is, with the current density. These observations were made with a wire serving as cathode.

With a disk cathode the author has previously shown that the cathode fall is a linear function of the current density, the gas pressure remaining constant. Stark, using a wire, finds it to be a parabola. He embodies in his results the condition that the current density cannot, as described above, fall below a certain value, which we may designate as the *critical* density. With a disk, the author finds this critical density to be a remarkably uncertain quantity, though the relation between cathode fall and current density does not vary with these changes. This characteristic is especially marked at gas pressures below 1.5 mm. In nitrogen the critical density varies (with the same gas) from about $0.2\,p$ milli-amp./square cm. as a maximum value, to *less* than $.01\,p$ as a minimum — this lower limit being that of the milli-ammeter. At higher pressures the critical density is larger and fluctuates less — being about $0.3\,p$ at 4.5 mm.

Simultaneous observations with two cathodes of the same material, one a disk and the other a wire, showed the cathode fall to be practically the same when the cathode current density was the same, but the critical current density for the wire to be about one and a half times the maximum value for the disk. By manipulation the critical density for the wire also was reduced to less than one half its normal value (at a gas pressure of 0.85 mm.) in which case the cathode fall was reduced accordingly.

These results disprove the supposition that the critical current density represents the minimum cathode fall obtainable, that the fall would increase if by any means we should be able to force the current density below this value. It may be caused by the resistance of the gas under the influence of the cathode increasing with decreasing current density, as shown by the author. According to the latter view the critical density would be that for which the potential difference between the cathode and a point in the stream at the boundary of its influence remains a minimum as the current varies.

ARE FALLING BODIES DEFLECTED TOWARD THE SOUTH?[1]

BY E. H. HALL.

IN *Science* for November 29, 1901, Professor Cajori published an article under the heading "The Unexplained Southerly Direction of Falling Bodies," in which he gave a summary of the experimental results obtained by the various observers of this deviation. The main parts of this summary can be put into tabular form as follows: S. D. standing for southerly deviation, and E. D. for easterly deviation:

Observer.	Time.	Place.	Fall.	S. D.	E. D.	S. D. ÷ E. D.
Hooke,	1680	London	8.2 m. (?)	+	+	?
Guglielmini,	1791	Bologna	78.3 "	1.19 cm.	1.89 cm.	0.63
Benzenberg,	1802	Hamburg	76.3 "	0.34 "	0.90 "	0.38
Benzenberg,	1804	Schlebusch	84.4 "	0. "	+	0.
Reich,	1831	Freiburg	158.5 "	0.44 "	2.84 "	0.16
Rundell,	1848	Cornwall	400 "	(25 to 50) cm.	?	?

To this table the author now adds from his own experiments,

1902	Cambridge	23 m.	0.0045 cm.	0.149 cm.	0.03,

the probable error of the S. D. being nearly as large as the value found for the deviation itself.

Gauss and Laplace have by theoretical discussion arrived at the conclusion that the S. D. for experimental heights is inappreciable. It is therefore worth while to examine carefully the original accounts, so far as they are available, of the various experimental researches regarding this deviation, to see whether any explanation of the discrepancy between theory and observation can be found. The most thorough examination which I have been able to give of these researches does not yield any satisfactory explanation. The testimony from the work of any one experimenter in favor of a southerly deviation is not entitled to much confidence, but the general agreement of the testimony that there is such a deviation makes further experiments desirable.

My own results were obtained from 948 trials made under a considerable variety of conditions, but the distance available was too small for the most conclusive results.

THE CHANGE OF ATMOSPHERIC NUCLEATION DURING COLD WEATHER.[1]

BY CARL BARUS.

THE author has continued the work with his coronal method of counting atmospheric nucleation, and shows the remarkably high values obtained during the very cold weather of December and January. The ten-

[1] Abstract of paper read before the Physical Society at the meeting held on December 31, 1902.

dency of the nucleation to increase cotemporaneously with a rapid fall of temperature and to fall off with rise of temperature is particularly brought out in a series of parallel curves. The author observes that fall of temperature must also be favorable to the formation of water nuclei and three hypotheses are advanced: A current from the upper air regions rich in nuclei may be brought to the observer by the cold wave; or the formation of water nuclei may bring down an air stratum overlying cities; or the water nuclei may be radioactive. Special experiments are in progress to test the latter question.

Ionization and Nucleation of the Phosphorus Emanation.[1]

By Carl Barus.

THE author endeavors to ascertain whether in an emanation neutral as a whole like that from phosphorus, evidence may be obtained that the negative ions are more efficient as condensation nuclei than the positive ions: for in such a case the emanation after successive partial precipitation in the condensation chamber, should become continually more positive. The results however show that the ionization vanishes practically in a few minutes, while the nucleation is *by contrast* indefinitely persistent and constant. With water nuclei it may be demonstrated that before and after the time in which the ionization nearly quite disappears, the coronas scarcely change in character. Hence in these experiments no relation between ionization and nucleation can be detected.

Experiments with a Holtz Machine and the Ratio of the Velocities of the Ions.[1]

By Carl Barus.

BY attaching a point electroscope, adjusted both to dissipate charge and to measure the potential at which the dissipation takes place, alternately to the positive and the negative electrode of a Holtz machine, the author finds from the potentials observed on open circuit, V_p' and V_n' and from the residual potentials of the closed circuit, V_{rp} and V_{rn}

$$(v_n/v_p)^2 = V_p'(V_{rn} - V_n')/V_n'(V_{rp} - V_p')$$

where v_n/v_p is the ratio of the negative and positive ionic velocity. The data showed $v_n/v_p = 1.32$, agreeing very well with the most approved values.

[1] Abstract of a paper presented at the meeting of the Physical Society held on February 28, 1903.

THE RÔLE OF THERMOELECTROMOTIVE FORCES IN A VOLTAIC CELL.[1]

BY HENRY S. CARHART.

THE writer presented briefly the thermo-dynamic theory of a voltaic cell, so far as relates to its properties dependent on temperature. It was shown that all these could be completely explained by means of electrolytic thermoelectromotive forces between a metal and the liquid in contact with it. Thermoelectromotive forces exist without temperature difference at the junctions, for a current will either absorb or generate heat at a junction according to its direction in relation to that of the thermoelectromotive force there.

Data were given showing that the temperature coefficients of a Daniell cell, a Carhart-Clark cell, and a calomel cell are all accounted for numerically by the thermoelectromotive forces at the metal-liquid junctions.

It was also shown that the heat represented by the second term of the Gibbs-Helmholtz equation is the difference between the heat generated at the negative electrode, where the current flows against the thermo-electromotive force, and that absorbed at the positive, where both current and electromotive force are in the same direction. The effects are thus localized in the cell.

It was also demonstrated by curves and numerical data that the electromotive force of a concentration cell is explained for dilute solutions by the thermoelectromotive force at the two electrodes, because this electromotive force increases with the density of the solution. For this last reason also thermoelectromotive forces explain the change in the electromotive force of a Daniell cell when the density of either solution is changed. All these conclusions have been confirmed by numerous measurements.

ON THE MECHANICAL EFFICIENCY OF MUSICAL INSTRUMENTS AS SOUND PRODUCERS.[2]

BY A. G. WEBSTER.

EXPERIMENTS were made on the efficiency for the tone of frequency 256 of the following instruments: Cornet, French horn, bombardino (brass), saxophone, clarinet, oboe (reeds), baritone voice, and violin. For the wind instruments, the pressure was measured by a

[1] Abstract of a paper presented at the meeting of the Physical Society held on February 28, 1903.

[2] Abstract of a paper presented at the meeting of the Physical Society held on December 31, 1902.

water gauge connected with a tube in the corner of the mouth. The volume delivered to the instrument was obtained by measuring the air inspired from a spirometer and that expired to the same after blowing for a measured period. For the voice, the pressure was estimated by singing steadily, closing the mouth on the gauge while maintaining the same muscular effort on the lungs. From pressure, volume and time the input of energy was obtained. For the violin, the bow was supported so as to be played mechanically by a measured pull with a measured velocity. The output in sound was measured by the author's phonometer, by comparison with the output of the standard "phone" placed in the position occupied by the instrument. Preliminary results follow. The small efficiency of the oboe is due to the fact that most of the work goes to the production of harmonics, which are not here taken account of.

	Pressure.	Volume / sec.	Energy Input.	Sound Output.	Efficiency.
Cornet.	13 cm.	52.6 $\frac{\text{cm.}^3}{\text{sec.}}$	.067 watts.	770 $\frac{\text{ergs}}{\text{sec.}}$	.00115
French horn.	18.2	70.0	.125	4,734	.0038
Bombardino.	17	58.1	.097	12,310	.0127
Saxophone.	22	102.5	.222	1,970	.00086
Clarinet.	16	46.7	.073	3,076	.0042
Oboe.	25	24.0	.059	31	.00005
Voice.	6	199.0	.117	11,070	.0095
Violin.			.048	254	.00052

Diffusion and Supersaturation.[1]

By Harry W. Morse and George W. Pierce.

THIS is an account of a research on diffusion and supersaturation in gelatine solutions, by a method based on Liesegang's "A-lines." When the end of a capillary tube containing a gelatine solution of potassium chromate is dipped into a water solution of silver nitrate, the silver nitrate diffuses up into the tube and throws down a precipitate of silver chromate. The silver chromate, instead of growing continuously as diffusion proceeds, forms in distinct layers widely separated in comparison with the thickness of the layers.

By means of the mathematical theory of diffusion and the equation expressing the equilibrium of the silver ion, the chrom ion and the undissociated silver chromate, the authors have calculated the extent to which experimental data necessary are the distances of the respective layers from

[1] Abstract of a paper presented at the meeting of the Physical Society held on February 28, 1903.

it is possible to supersaturate a solution with silver chromate. The only the bottom of the tube and the times at which they form. By employing various initial concentrations of the diffusing substances it is found that the concentrations of the ions at the limit of supersaturation satisfy a formula similar to the corresponding formula for saturation in the presence of the solid phase, namely,

$$\overset{+}{\mathrm{Ag}}{}^2 \times \bar{\mathrm{C}}\mathrm{r}\bar{\mathrm{O}}_4 = \mathrm{H},$$

in which $\overset{+}{\mathrm{Ag}}$ is the concentration of the silver ion, $\bar{\mathrm{C}}\mathrm{r}\bar{\mathrm{O}}_4$ the concentration of the chrom ion, and H a constant called the "Metastable Solubility Product" in distinction to the "Solubility Product" for saturation in the presence of the solid phase.

The value of H obtained for silver chromate is 1.4×10^{-6}. The ordinary solubility product is 5.1×10^{-13}. By taking the cube root of the ratio of these numbers it is seen that at the limit of supersaturation the solution contained 145 times as much silver chromate as is required to form a precipitate in the presence of the solid phase. It was found that the metastable limit is a perfectly definite quantity. So far as the authors know, the numerical value of the metastable limit has not previously been published for any substance.

Incidentally the diffusion constant for silver nitrate in gelatine was determined.

The method is applicable to the determination of the diffusion constant and the limit of supersaturation of a great variety of slightly soluble substances.

JEFFERSON PHYSICAL LABORATORY,
HARVARD UNIVERSITY.

A SIMPLE GEOMETRICAL PRINCIPLE AND ITS POSSIBLE SIGNIFICANCE IN CONNECTION WITH A GENERAL PHYSICAL THEORY.[1]

BY JOHN MILLIS.

THE various possible arrangements of a collection of spherical bodies of equal size are considered and the employment of "ping-pong" balls fastened together by adhesive material for experiment and illustration is suggested.

The arrangement commonly regarded as the most compact possible, when the balls are in contact throughout and the mutually tangent planes form the oblique dodecahedron about each ball, is capable of indefinite extension. In such a collection any one ball may be taken as the cen-

[1] Abstract of a paper presented at the meeting of the Physical Society held on February 28, 1903.

tral one of a group of thirteen and the twelve balls tangent to it will have the same disposition in all of such groups, being of course tangent also to each other. The arrangement is a rigid one throughout but not entirely symmetrical or homogeneous as the void spaces have two general forms, one form of space being surrounded by four balls and the other by six. The above arrangement is called arrangement *A*.

Twelve balls may also be placed about and tangent to a central one so that no two of the side balls are in contact with each other, the mutually tangent planes forming about the central ball the regular dodecahedron. This is called arrangement *B*. It cannot be extended on the same system beyond thirteen balls as a simultaneous condition, but in a collection of an indefinite number each ball may become temporarily and in turn the central one of a group having arrangement *B* by proper relative movements throughout the mass.

If to each of the two groups of thirteen balls having arrangements *A* and *B* one additional layer of balls is added by placing a ball over each interval on the outer surface of the collection, group *A* will have twenty-seven balls and group *B* thirty-three. The circumscribing tangent sphere for the twenty-seven will have a greater diameter than the circumscribing tangent sphere for the thirty-three, and all the balls in this latter group except the central one may be moved radially so the balls are nowhere in contact with each other and they can still be included in a tangent sphere smaller than that enclosing the twenty-seven, notwithstanding these latter have the arrangement commonly considered the most compact possible. By removing the central ball of the thirty-three group the remaining thirty-two may be included in a sphere still smaller relatively than the one enclosing the twenty-seven group and yet have no two of the thirty-two group in contact.

A third arrangement, called arrangment *C*, is that in which the mutually tangent planes form cubes.

The assumption is now made that the balls come together in a collection under their mutual attractions according to gravity laws. They will not assume or remain in arrangement *C* because while this gives symmetry throughout it is not the most compact possible and the equilibrium of the collection would be unstable.

Arrangement *A*, while the most compact possible for an indefinite number of balls in contact throughout, is not entirely symmetrical.

Arrangement *B* gives a perfectly symmetrical disposition of twelve balls with respect to a central ball but it is geometrically impossible throughout a collection of a greater number than thirteen.

It is suggested that under the conditions assumed the result will be that the balls will assume no fixed arrangement but that they will be in continual relative movement, striving after the unattainable arrangement

that will give minimum total volume, symmetrical disposition, and therefore fixed stable equilibrium throughout.

In arrangement *B* if regular dodecahedrons be formed about the balls these solids will approximate to a "fit" but there will be of course vacant spaces, since the diedral angle of the regular dodecahedron is somewhat less than one third of three hundred and sixty degrees.

Similarly regular tetrahedrons formed by drawing right lines joining the centers of four balls placed as close as possible will not quite stack up without voids, though this condition is approximated to.

As an assistance to an understanding of conditions as they actually exist it is suggested that it might be interesting to speculate upon what the consequences would have been had it so happened that the diedral angles of the regular dodecahedron and the regular tetrahedron were exact fractional parts of three hundred and sixty degrees.

Reference is made to some apparent relations between the totals that result from adding layers of balls in arrangement *B* and the series of differences in the table of atomic weights when the numbers representing these weights are arranged according to the periodic law.

The simple geometrical principle referred to in the title is concisely stated as follows:

In an aggregation of an indefinite number of equal spherical bodies an arrangement giving minimum total volume and perfect symmetry throughout is impossible.

The possible significance of this in connection with a general physical theory is simply that it appears to offer an explanation — independent of any assumption of molecular repulsion or of a special medium or any special attribute of matter other than gravity — of the impossibility of the existence of a homogeneous plenum of matter. From this follows a possible elucidation of the vexed problem of elasticity in its various forms and consequently more or less advance towards an understanding in nearly the whole range of physical phenomena.

No special assumption is made as to the shape or size of an ultimate particle of matter or whether there is in fact such a thing as an ultimate particle of matter. It is sufficient that matter having the attribute of gravity only cannot exist as a homogeneous plenum. If such a plenum were possible it would be possible to describe within it a system of equal spheres of any size, small of course with respect to the dimension of the body considered, not limited as to location, and arranged so that the matter in the volume not contained in the spheres could be removed leaving the spheres in a condition of fixed and stable equilibrium; and conversely so that the spheres could be removed leaving the remaining matter in the same condition. This cannot be done.

It is suggested that the above may indicate some progress towards a

generalization in which all physical phenomena are reduced to the irresolvable conception, *time*, *space* and *matter-gravity*.

SEATLLE, WASH.,
February 21, 1903.

A METHOD OF DETERMINING INTERNAL RESISTANCE, APPLICABLE TO RAPIDLY POLARIZING CELLS.[1]

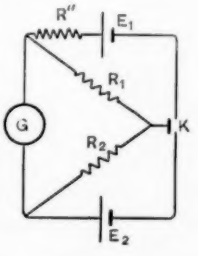
BY J. R. BENTON.

THE methods of determining internal resistance may be divided into two classes: (1) Those in which the current through the cell is varied, and (2) those in which it is kept constant. Methods of the first class cannot be used with cells which polarize rapidly, for in such cells the internal resistance is a function of the current and of the time it has been flowing.

Some of the methods of the second class have the disadvantage that they require an instrument for measuring current or difference of potential, whose constant must be known. The method of Beetz, which can be used with rapidly polarizing cells if they are in circuit only for short intervals of time, avoids this inconvenience, but has several other disadvantages.

The method about to be described is similar in principle to that of Beetz, but appears to possess some advantages as compared with it, as follows: (1) There is no limitation to the E.M.F. of the cell to be investigated; Beetz's method cannot be used unless this E.M.F. is greater than that of the smallest available auxiliary cell. (2) The method can be used for cells of very low internal resistance; Beetz's method cannot be used to advantage under such circumstances. (3) While the accuracy of the results must depend in either method on a gaeat many different considerations, it seems that in general this method gives greater accuracy with a galvanometer of the same sensitiveness.

The apparatus required is the same as in Beetz's method, except that an additional resistance box is needed. The connections are shown in the accompanying diagram. At K is a key which can be thrown so as to complete at the same time the circuits $KE_1R''R_1$ and KE_2R_2; E_1 is the cell whose resistance is to be determined; E_2 is an auxiliary cell; G a galvanometer, preferably of low resistance and sensitive; R_1, R_2, and R'' are resistance boxes.



[1] Abstract of a paper presented at the meeting of the Physical Society held on February 28, 1903.

The determination is made as follows: Taking all the resistance out of R'', so that its resistance is zero, set R_1 at a convenient small resistance; then adjust R_2 so that the galvanometer gives no deflection when the key K is thrown. Next set R_1 at a large resistance; then, keeping R_2 unchanged, adjust R'' till no deflection of the galvanometer occurs when the key is thrown. Denoting the first value of R_1 by R_1, the second by R_1', and the resistance in R'' by R'', we have the internal resistance of the cell equal to

$$\frac{R_1 R''}{R_1' - R_1}$$

NEW BOOKS.

A Laboratory Manual of Physics for Use in High Schools. By HENRY CREW and ROBERT R. TATNALL. Pp. 234. New York, The Macmillan Company, 1902.

A Manual of Elementary Practical Physics (second edition). By JULIUS HORTVET. Pp. 276. Minneapolis, H. W. Wilson, 1902.

Laboratory Exercises in Physics for Secondary Schools. By GEORGE R. TWISS. Pp. 193. New York, The Macmillan Company, 1902.

As we pass on to a stage of fuller provision for the needs of teachers and pupils in secondary schools, three types of books must be furnished in connection with the instruction in physics. These will correspond to three elements now generally recognized as necessary to a sufficiently broad presentation of the subject — at least where large classes are taught. First, a collection of illustrative experiments, to be carried out by the teacher before the class, introducing fundamental relations among phenomena (more usually on their qualitative side) with greater variety than the scanty laboratory time allows. Secondly, the laboratory manual proper, designed as a guide for the individual experimental work of the pupils (mainly quantitative). And, thirdly, the text-book as a basis for discussion in the class-room. It may be objected that a capable teacher should devise his own demonstrative experiments; but it remains practically true that there is still a field for a book which shall gather up and record the dispersed traditions of many class-rooms for the common benefit of all. Such an enterprise would not be so remunerative as a successful text, but it would be a piece of public service to compile the data.

The three books here given by title are intended to fall into the second class; and all of them possess distinctly the merit of putting into circulation suggestions of practical teachers, tested and tried out under working conditions. Any one of the three offers a treatment that will be judged satisfactory of those laboratory exercises which, under slightly varying forms, are becoming standard material. The choice among these manuals will then be determined by other features, expressing more particularly the points of view of their authors. Thus the "Laboratory Exercises" (Twiss) announces a consistently limited purpose — it is written for the *pupil* and the *laboratory*, leaving the teacher's experiments

and the text for the class-room to be supplied from other sources. In the main, too, it offers one set program of forty-three exercises for a (full) year's work.

The "Elementary Practical Physics" (Hortvet) is more composite in its selection of material, and includes so much explanation introductory to the experiments that it may be said to occupy ground half-way between text and laboratory manual. Here, again, we have a "course" including about forty exercises. To be sure, if the text-book used as an accompaniment is too abridged, it is desirable to supplement it; yet, assuming a good text, a short and disjointed statement of theory in a laboratory manual is likely to confuse the pupil rather than to satisfy him. On matters more closely related to the laboratory, such as sources of error, and figures "significant" of accuracy in the result, this book gives many good suggestions.

The "Laboratory Manual" (Crew and Tatnall) shows commendable unity of purpose in avoiding any considerable overlapping on the field of the text-book. It is also, to some extent, fresher and less stereotyped in its forms of experiment. And, with the possibility opened that the "Teacher may select, say, thirty or sixty out of a list of more than ninety exercises," the suggestion can be taken that the remaining experiments may be recast (and perhaps expanded) into a set of class-room demonstrations, making the manual serve a legitimate double end — aiding both teacher and pupil — until the book of directions for the former comes to be published.

It is to be regretted (to express a personal opinion) that "measurements for the measurements' sake" still hold any place in the laboratory training of beginners. Every hour of their time should be made to count for the cultivation of interest in physical thought and methods, and of insight into common physical phenomena. Neither of these ends is noticeably served by an exercise "to measure a length," or "to weigh by oscillations," or "to find the ratio of the inch to the centimeter"—taken alone; is it not agreed (pedagogically) that the manipulation of such measurements is better learned where they enter into a combination of *physical* interest? I should like, also, to raise continually the question, for consideration by teachers of elementary physics, whether the formal and abstract ideas of mechanics are best put at the threshold of the subject? Must the completed logic of the C.G.S. system forever determine the line of approach to the exclusion of a more experimental topic, like heat or light?

FREDERICK SLATE.